

Psychological Review

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THE PSYCHOLOGICAL REVIEW

CAN TOLMAN'S THEORY OF LEARNING HANDLE AVOIDANCE TRAINING?

BY CHARLES E. OSGOOD

University of Illinois

It might be wiser to keep the contents of this paper buried in some hidden file where no Tolmanite could find it. The writer both through training and conviction is partial to Hull's theory of learning, yet he is aware that Tolman's theory can take care of the phenomena of avoidance. The incorporation of these phenomena, however, within an expectancy theory is not an easy matter. The problem first turned up in connection with a critical evaluation of contemporary learning theories, and it looked as though a crucial point *against* Tolman's interpretation had been discovered. At it turns out, Tolman's theory can handle this situation very neatly. Since the bearing of avoidance training phenomena on Tolman's position has not been studied intensively to date, this brief analysis may prove worthwhile.

The experimental situation is best illustrated by the now classic study by Brogden, Lipman and Culler (1) comparing Pavlovian conditioning with avoidance training. Guinea pigs were placed in a revolving cage. A buzzer, sounding for two seconds, served as the conditional stimulus, this being terminated by a shock, the unconditional stimulus. The animals made various responses to the shock, amongst which

would be running in the cage. Under Pavlovian procedures the animals were shocked on every trial, regardless of whether the "conditioned" running response to the buzzer was made. Under avoidance training the animals escaped shock entirely if they ran while the buzzer was sounding. While avoidance subjects reached a level of 100 per cent anticipatory running by the eighth day of training, non-avoidance subjects showed no consistent improvement in performance, conditioned runs occurring on only 20 per cent of the trials even after twenty days of training.

Although these results are what would be expected on a common-sense basis, they are superficially embarrassing to reinforcement (Hullian) and contiguity (Guthrian) positions. As to the reinforcement principle, the regularly shocked animals are reinforced (pain drive reduction) on every trial, while the avoidance animals receive no apparent reinforcement when they successfully avoid shock—it would appear that absence of shock stimulation is somehow reinforcing. Mowrer (5, 6) has demonstrated how reinforcement theory can incorporate these data, essentially by invoking secondary (anxiety) motivation. As to the contiguity principle, if the consistent response to shock

is running, the non-avoidance animals should do at least as well as the avoidance animals. Sheffield (7) has recently shown how Guthrie's theory can be applied here, essentially by demonstrating experimentally that further running is not the consistent response to shock when it is given during a conditioned run. These interpretations need not concern us further here.

The belief that an expectancy theory, such as Tolman's, can easily handle this kind of situation is prevalent. Superficially, if an animal comes to expect pain or punishment in a certain situation, it will behave appropriately by some avoidance movement. This belief has undoubtedly been fostered by Hilgard and Marquis' (4) influential review of learning literature, where the avoidance training experiments of Culler and his associates (1, 2, 3) are closely linked with Tolman's theory of sign-learning. Thus they say (4, p. 89), "The guinea pigs in the revolving cage of Brogden, Lipman, and Culler must learn that at the sound of the buzzer the cage is potentially 'dangerous,' i.e., a shock is likely to occur, and that the shock is less likely to occur if they start running when the buzzer sounds." They also state (in Table 2 on page 99) that the "expectancy principle is directly applicable," i.e., without any devious reasoning, to the avoidance training situation. Neither Tolman nor anyone else working within the framework of his theory has tackled this experimental phenomenon directly.

It is unfortunate, especially in cases of this sort, that Tolman has never given rigorous formal statement to his system. In order to determine whether or not his theory covers a given situation, it is first necessary to state his postulates in some explicit manner. White (10) has made a step in this direction by phrasing two principles for

Tolman's theory as follows: (1) PERCEPTUAL-LEARNING POSTULATE. *When a particular piece of behavior in a particular situation is once perceived as a path to a particular object . . . , a more or less permanent "knowledge" of this relationship usually results.* (2) PATH-GOAL POSTULATE. *If there is a motive or "need," the goal of which is a particular object . . . , and if at the same time there is available the knowledge that a particular piece of behavior is a path to that goal-object . . . , then that behavior will tend to occur.* To apply these postulates to the avoidance training situation, we must translate "particular piece of behavior" as "running response," "goal-object" as "avoidance of shock," and we would add a translation of the term "sign" (which is important in Tolman's theorizing, even though omitted in White's version) as "buzzer." In the Brogden *et al.* study, the avoidance animals presumably establish the cognitive "bit of knowledge" that running is a path to the "object" of non-shock and, having a "need" to avoid shock, continue to execute this "particular piece of behavior."

From his own study of Tolman's writings, the present writer concludes that at least four postulates (excluding all capacitance laws) are necessary. They are stated here for comparison with White's version and to facilitate a more rigorous analysis of avoidance training. Wishing to give this note the brevity it deserves, it must be assumed that the reader is reasonably familiar with Tolman's terminology.

(1) MOTIVATIONAL POSTULATE. *Demands selectively sensitize those means-end-readinesses and sign-significate-expectations with which they are associated.* The "demand-against-shock" in the avoidance situation sensitizes those general sets (means-end-readinesses) for running away, climbing out, and so

forth that have been associated innately or through previous learning with pain; if the animal is "shockwise," the specific expectation that alternately lifting the paws will reduce pain might be sensitized as well. (2) ASSOCIATION POSTULATE. *Whenever one stimulus-situation (sign) is followed by another (significate), there is established a relation between them such that on subsequent occasions the former gives rise to an expectation of the latter (sign-significate-relation).* The buzzer, being followed by the shock, becomes a sign of the shock, *i.e.*, subsequent occurrences of the buzzer tend to give rise to the expectation that shock will follow. This statement roughly parallels White's first postulate. (3) STRENGTHENING POSTULATE. *Sign-significate-expectations, as relational associations, are strengthened by confirmation and weakened (disrupted) by non-confirmation.* During the early trials for both avoidance and non-avoidance animals, the expectation that shock will follow buzzer is consistently confirmed—the shock does in fact follow the buzzer—and hence this expectation is strengthened. On trials when the shock is successfully avoided by the avoidance animals, this expectation that buzzer will be followed by shock is not confirmed and hence weakened. This postulate is notably absent from White's version; in fact, his first statement implies an all-or-nothing, single-experience establishment of cognitions, which is not at all Tolman's view as I understand it (*cf.* Tolman, 8, p. 386; Tolman and Brunswik, 9). (4) ACTION POSTULATE. *Specific sign-significate-expectations, as cognitive events, are released by sign-stimuli and, when demand is also present, mediate overt behaviors "appropriate to" sign-stimuli.* Given the expectation that shock will follow buzzer, and having a demand-against-shock, the guinea pig

releases "appropriate" responses, such as leaping, crouching or running. This postulate is essentially identical with White's second law.

We are now in a position to analyze the Brogden, Lipman and Culler experiment more carefully in relation to Tolman's system. For both avoidance and non-avoidance subjects the buzzer becomes a sign for expected shock (Post. 2) and this cognitive expectation is strengthened by confirmation during the early trials (Post. 3). Being a condition innately producing physiological disturbance, the shock releases a "demand against" in the animals, this demand in turn sensitizing various means-end-readinesses, learned and innate, which eventuate in appropriate behaviors (Posts. 1 and 4)—hence running, crouching and so forth. Now, if any *new* expectation is to be established, the animal must experience a new stimulus sequence (Post. 2), and this is possible for the avoidance subjects. Since their running responses are followed by non-shock, they establish the new expectation that running-to-buzzer signifies non-shock (Post. 2) and this new expectation is consistently strengthened by confirmation—running does in fact eventuate in non-shock for them (Post. 3). Given the demand to avoid shock and the mediating cognitive expectation that running signifies non-shock, the appropriate running response is repeatedly released by the buzzer (Post. 4). For the non-avoidance subjects, of course, no new expectation involving escape from shock can be established. So these animals merely keep on expecting shock—and this expectation keeps on being confirmed.

So far the easy and direct explanation of avoidance training in terms of expectancy theory has simply been made more explicit. But things are not as easy as they seem. Sheffield's recent

study (7), duplicating the Brogden *et al.* conditions in all essentials but recording behavior more accurately, revealed a significant fact. The avoidant running response was found to *weaken* gradually (in terms of both latency and amplitude) throughout those series of trials where the shock was successfully avoided, until, delaying too long, the animal again got shocked. Similar observations were made by Mowrer (6) under equivalent conditions. In Tolman's terms this would seem to indicate that the new expectation (that running-to-buzzer signifies no-shock) is being weakened during trials on which no shock is received, since the mediated running behavior becomes slower and less vigorous. But how can this be, when this expectation is consistently being confirmed (Post. 3)? Running does in fact lead to non-shock every time. It would appear that the very phenomenon following most easily from his theory is to disprove it. For here, indeed, is a case where a consistently confirmed expectation becomes weaker.

This situation must now be inspected more carefully. The weakening of the overt running behavior during successful avoiding trials is an objective fact. What can Tolman's system do with it? There seem to be two alternatives: Either the new shock-avoiding expectation must be one which is *not* confirmed (to explain the gradual weakening of mediated running) or the "demand-against-shock" must be diminishing during these successful trials. The first alternative is obviously untenable—such an unconfirmed expectation could never have been established in the first place.

The second alternative offers interesting possibilities. According to Tolman, actual release of overt "pieces of behavior" is some *multiplicative* function of cognitive "knowledge" and "demand"

(*cf.* White, 10, p. 171). With the expectation that running-to-buzzer signifies non-shock maintained at constant strength, the vigor and speed of overt response could gradually diminish to zero if the "demand-against-shock" were gradually reduced to zero. The problem for Tolman would be to discover some good reason within his postulates for anticipating such a reduction in "demand-against-shock" during trials on which shock is successfully avoided. There is an excellent reason: Once the animals *do* begin to successfully avoid shock by running, the original expectation—that buzzer signifies coming shock—is no longer confirmed and it therefore weakens progressively (Post. 3). Tolman would probably accept a statement to the effect that this "demand-against-shock" (or "fear" of shock) is itself a state released by the buzzer—as long as the buzzer remains a sign of coming shock. But, as has been shown, during the successful avoidance trials the expectation that painful shock will follow the buzzer must be weakened by non-confirmation, since the buzzer is not in fact followed by shock (Post. 3). Therefore, the "demand-against-shock" must decrease during the successful avoidance trials and the mediated running behavior weaken correspondingly (Post. 4).

So Tolman's system proves capable of handling avoidance training phenomena, granting that the formalization of his basic assumptions given above is adequate. There are "soft" spots in the analysis, of course. For one thing, one wonders how a *lack* of stimulation can function as a significate-object (*i.e.*, how can *non-shock* be an expected significate?), at least for non-linguistic organisms. Tolman generally speaks of both signs and significates as being patterns of stimuli. For another thing, the relation between cognitive expectations

and the responses they mediate remains obscure here as it is in all Tolman's work. In this connection it should be kept in mind that expectations are hypothetical constructs and are necessarily inferred from overt behavior—this critical step in the inferential sequence badly needs elucidation. Finally, the precise mechanism whereby "demands" of this type become modified in strength has not been clarified. It has been assumed that "demand-against-shock" here operates much as an acquired anxiety drive in Mowrer's interpretation.

As was pointed out earlier, both reinforcement (*cf.* Mowrer) and contiguity (*cf.* Sheffield) principles have been shown capable of incorporating these avoidance phenomena, so this is not a crucial test among theories. However, the expectancy explanation does seem to have one advantage: Since the expectation that running-to-buzzer signifies non-shock, as a cognitive "bit of knowledge," retains its full strength by continuous confirmation, and since a single experience with actual shock is presumably sufficient to rejuvenate completely the "demand-against-shock," as another cognitive state, this system is able to explain the immediate return of running at or near full strength after a single experience of dallying until shocked. Reinforcement and contiguity theories would probably have some trouble on this point. Of course, the analysis presented in this paper implies no evaluation of the status of Tolman's theory *qua* theory-in-general. There are many criticisms that can be aimed at

the expectancy theory as a whole, especially with respect to quantifiability and the indexing of variables. Possibly those who hold to the Tolman-type theory will look upon this attempt to state postulates and apply them as a small step in the direction of increased rigor, even though the step was made from the "enemy camp"—which is a primitive notion for natural scientists.

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[MS. received September 23, 1949].

COLOR VISION AND FACTOR ANALYSIS: SOME COMMENTS ON COHEN'S COMMENTS

BY F. NOWELL JONES

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In the July issue of this JOURNAL, Cohen (1) has written an interesting article in which, among other things, he has some comments to make about my factor analysis of visibility data (2). Since I believe that his comments are not really germane to the purpose of my analysis, I shall here state in perhaps greater detail what it was that I was attempting to do. Before proceeding to more basic issues, however, it might be well to mention two minor points raised by Cohen (1, p. 230-231).

The factor loading for 559 millimicrons on factor IV is an obvious printer's error. At some point .069 became converted into .609. Also, although I was somewhat remiss in not stating more explicitly the final position into which the axes were rotated, it is indicated on p. 369 that rotation had been made into simple structure, which is indeed the case.

Probably the best way of discussing the bulk of Cohen's objections is to review, briefly, the purpose underlying the work which he criticizes. In a very general way the idea was to find out whether statistical attention to relative variability from person to person would yield meaningful results in a field ordinarily explored by the traditional psychophysical methods. As I believe was clearly indicated, the analysis of vision was undertaken to see whether or not factor analysis would generate hypotheses about vision which would be reasonable in the light of our present knowledge.

The factor analysis when finally completed tied in very well, it seemed to me, with three-process theory. What it

indicates is that the individual variability about the average visibility curve (as defined in the method used to collect the data) may be accounted for by three factors, and further that these three factors are a long-wave, a short-wave, and a middle-of-the-spectrum function. This would not mean, taken alone, that these three factors represent varieties of cone, of retinal pigment, or of any other specific structure. But in view of the amount of work already done in the field of vision, I did not, and do not, consider it unreasonable to point out how readily rationalizable these results are in terms of three-color, or, better, three-process theory. Judd's curves were chosen only because they represented well enough the general spectral areas covered by the three components of three-process theory, and because they were readily available. It would probably have been better to use curves for color mixture using real primaries. In any case the transformation from factor loadings to published curve could not be made directly, since I dealt with sensitivity and not with color matching or discrimination.

To understand fully the reason why I was, and am, gratified by the outcome of the analysis, one needs to imagine that no fruitful hypothesis concerning the number of independent variables needed to account for sensitivity had been proposed; in other words, let factor analysis function as the *hypothesis-suggesting* technique it really is. It would seem to me to be very obvious that the results of the analysis would lead directly to a three-process theory, and, furthermore, would place the three

processes in the proper regions of the spectrum. Only in this sense do I feel that my factors lead to Judd's curves.

Cohen points out that it is possible for two of the three luminosity coefficients in three-process theory to equal zero, hence leaving one curve the exact equivalent of the total luminosity curve. He proceeds from this to the conclusion that my results are impossible (2, p. 231). Probably his conclusion flows from his assumption that I think that my factor loadings *are* Judd's curves. They are not, of course, as I have agreed above. But they are not impossible either, since the analysis goes back to the data, not to a numerical transform which would, for convenience, set two of the three variables to zero. Starting with the fact that the S's varied, and that they varied from one another in systematic ways, as the many high correlations would suggest, factors have been found which suggest three processes as important in determining relative sensitivity. I do not see that Cohen has shown this to be impossible, especially since it has been done.

My general position here would be that if individuals differ systematically along a given dimension, or axis, then that dimension has meaning. In the case of vision the S's who participated in the experiment from which the data for analysis were taken varied very definitely in systematic ways. When it is considered further that analysis of this variability reveals factors which are in accord with current visual theory, then the analysis is of scientific interest.

This conclusion is further strengthened by the results of another analysis, which has just been completed, of visual luminosity data collected by an entirely different technique, and which shows substantially the same results. This analysis will appear elsewhere in the near future.

As a result of the considerations outlined above I am encouraged to proceed with the original purpose which prompted these visual analyses, namely, the exploration of olfaction. Since, in the field of vision, factor analysis yields results which would suggest the hypothesis of a three-process theory, I have hope that if there is anything systematic in olfactory thresholds as measured for a number of substances, the factors derived from analysis of the correlations among the substances will suggest hypotheses about the chemical factors, at present quite unknown, which underlie olfactory stimulation. Of course there may be no dimensions, but it would be expected that analysis in that case would resolve into a large number of specific factors.

In any event I can find nothing in Cohen's discussion which makes me willing to affirm the universal negative that factor analysis is of no use to psychophysics.

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LOGICAL RELATIONSHIPS BETWEEN MEMORIAL AND TRANSIENT FUNCTIONS

BY PAUL McREYNOLDS¹

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In the design of mathematico-mechanical analogies to the human central nervous system two fundamental types of problems may be distinguished. These are:

1) The problem of the transient processes correlated with behavior and mental activity. Psychological analogues are thinking, discriminating, judging, and all types of mental activity which go to make up the train of thought, and which have a phenomenally transient existence. Physical analogues are, or may be, communications procedures involved in, *e.g.*, teletype and computing machines, and servo-mechanisms involved in, *e.g.*, fire control devices (1, 7).

2) The problems of the symbolic retention of past events. Psychological analogues are all types of memory—remote memory, recent memory, and immediate memory span. Physical analogues are, or may be, condenser types of "memory" devices in certain automata, punched paper tape in teletype systems, sound recording devices, and printed materials³ (1).

¹ The content of this paper was developed during discussion with Fred Attneave, University of Mississippi. The author is indebted to William A. Cass, Jr., Stanford University and Veterans Administration, Palo Alto, for critical help in preparing the manuscript.

² Reviewed in the Veterans Administration and published with the approval of the Chief Medical Director. The statements and conclusions published by the author are the result of his own study and do not necessarily reflect the opinion or policy of the Veterans Administration.

³ Transient and memorial functions, as used here, appear to have much in common with Koffka's (2) *process* and *trace*.

In terms of the information dealt with the first problem is essentially one of communications, in the sense discussed by Shannon (4, 6) and Wiener (7). The second is primarily a matter of providing for the future re-communication of present information. How these problems are met by the CNS—and it is not implied that they are separable problems except for purposes of analysis—subsumes the questions of universals, dimensions of perception, dynamic memory alterations, and the retention "units" involved. It is to be noted that transient data exist in time, and may have a meaningful pattern over time, whereas the meaning of memorial data is independent of time. In psychoneurology transient processes may possibly be represented by discharges across certain synapses; memorial processes may possibly be based primarily upon relatively permanent changes in threshold at certain synapses. For our present analysis we shall not consider the many complications of neurons and synapses.

Our major purpose in this paper will be to note certain relations between the two types of problems rather than to discuss either of them in detail. As will be evident in our discussion we are much indebted to McCulloch and Pitts (3), Shannon (4, 6), and Wiener (7), particularly with regard to their treatment of information. Our own orientation, however, will be primarily toward "meaning" rather than "information" (in the sense used by Shannon and Wiener).

If we disregard dissipation in the amount of information in the process

of transmitting the information, then the number of unique expressions of information $I_1, I_2, I_3 \dots I_n \dots I_r$, which can be transmitted by a given apparatus is a function of the number of unique elements $k_1, k_2, k_3 \dots k_n \dots k_r$ in the apparatus. If each k can transmit one and only one I , i.e., can be either "on" or "off," then $I_n = k_n$, $I_r = k_r$, and $N_I = N_k$. With N_k constant, however, N_I can be greatly increased if particular arrangements of $k_1, k_2, k_3 \dots k_n \dots k_r$ are used to represent different I 's, so that as a maximum $N_I = 2^{N_k}$. Thus when $N_k = 5$, $N_I = 2^5$, or 32. In other words, to be capable of representing a given N_I , N_k is at a minimum when each I is represented by a unique arrangement of $k_1, k_2, k_3 \dots k_n \dots k_r$, each of which may be "on" or "off," rather than by a specific k . For purposes of communicating unique expressions of information,⁴ then, such a system offers maximum efficiency. Such a system—which we shall term Condition A—is, e.g., the basis of teletype. In psychoneurology it is ideally suited to transient functions. The essence of the approach is that each transient datum ("thought," "judgment," etc.) may owe its uniqueness to the particular arrangement of discharges in a particular group of neurons.

If such is the case—and obviously it is only an hypothesis—then several implications are fairly clear.

1) *Pacing*. If transient functions are represented by unique arrange-

⁴ A "unique expression of information," I , as used here, should not be confused with the "bit," a unit amount of information as used by Shannon, Wiener, and others, and defined as the possibility of one choice from two alternatives. Thus in the example in which $N_k = 5$, $N_I = 32$, and each I indicates one unique expression of information, but represents, in amount of information, 5 bits, i.e., 5 dichotomous choices.

ments of discharges then it is important that these discharges occur with near simultaneity in order to maintain the uniqueness. An analogous problem is met in teletype apparatus. Proper pacing of non-contiguous discharges could be regulated by a set of neurons providing periodic non-specific facilitation. This may be a function of the alpha rhythm. Such a mechanism would make unnecessary contiguous connections of all synapses involved in any given I . There is, of course, no need to assume any direct likeness between transient patterns and the experience they represent. Such transient patterns may be thought of as symbols, and retention units as symbols of symbols.

2) *Moments*. If unique transient functions are represented by unique discharge patterns paced by an overall regulator, then such processes must occur discretely, and therefore in sequence. This leads to the hypothesis that the proper analogy for transient data is not a "stream of consciousness," but rather a sequence of discrete units. After Stroud's (5) earlier formulation of substantially the same hypothesis (though on different grounds) these may be referred to as moments.

3) *"Transient Traces."* We wish merely to point out—without further development at this time—that if the above points are valid then the succeeding moments cannot be assumed to be completely independent of each other, for the discharge pattern involved in any given moment will influence to some extent the various thresholds which would determine to some extent the succeeding moment, i.e., an interaction must be postulated to exist between successive moments.

In memorial problems Condition A would not be particularly efficient. Let I' be any unique expression of information which may be retained, and

k' any apparatus unit involved in retention. Then, under Condition A, $N_{k'} = N_I \cdot 2^{N_k}$, *i.e.*, a duplicate apparatus as described above would be required for each I' . A more efficient apparatus for retention would exist when each k' stands for a certain specific I' , *i.e.*, when $I'_n = k'_n$; in this case $N_{k'} = N_I$. Let this kind of system be Condition B.⁵ The essence of this approach is that each "retention unit" may owe its uniqueness to a particular unit neural change—presumably, as an ultimate in psychoneurology, relatively permanent change of threshold at a single synapse.

To summarize our discussion to this point, it is clear that for transient functions $N_I = N_k$ under Condition B and $N_I = 2^{N_k}$ under Condition A. Thus with N_I constant N_k is less in Condition A than in Condition B. For this type of problem Condition A is therefore more efficient than Condition B, *i.e.*, fewer apparatus units are required to express a given N_I . For memorial problems the situation is quite different. Here, for Condition A, $N_I = N_{k'}/2^{N_k}$, and for Condition B, $N_I = N_{k'}$. Obviously, for a given N_I , $N_{k'}$ is less in Condition B than in Condition A. For memorial problems Condition B appears, therefore, to be more efficient than Condition A, *i.e.*, fewer apparatus units are required to retain a given N_I . On this basis it is apparent that with *either* Condition A or Condition B existing for *both* transient and memorial functions the efficiency for one type is maximal and for the other type minimal.

⁵ It may be noted that both Conditions A and B are digital systems. Condition A is a binary system; in Condition B the numerical base is N_I . Thus if N_I were 10, the ordinary decimal system would be an example of Condition B. The two conditions represent extremes, of course. The number of possible conditions is $N_I - 1$ and the Condition at any given time may itself be a neural variable.

The situation with regard to memorial problems must, however, be examined further. Under Condition A *each* hypothetical neurological retention unit can retain *any* I' , but for each such unit a great many synapses will be required— 2^{N_k} of them, in fact—and therein lies the inefficiency of Condition A. Under Condition B, on the other hand, each hypothetical neurological retention unit can retain only one certain I' , but for each such unit only one—or a very few—synapses need be involved. Since this obviously requires a high degree of specificity of some kind—presumably of location—we are faced with what appears to be the necessity of postulating a sufficiently large $N_{k'}$ in reserve to provide a particular k' for each possible I' . The inefficiency of Condition B lies in the necessity of such a reserve. Clearly the relative efficiency of Conditions A and B for memorial problems is a function of the proportion of N_I , which is actually to be retained, Condition A being the better when the proportion is small and Condition B the better when the proportion is large.

With present neurological knowledge there is no conclusive reason to favor either Condition over the other for memorial problems. It is possible, however, to make certain tentative general statements about the kind of memorial system which would have maximum efficiency. In such a system only one or a very few apparatus units would be required for each I' ; the organization of the $N_{k'}$ units would provide for possible random connections, *i.e.*, would not be completely dependent upon innate organization; a relatively high proportion of the $N_{k'}$ units would not be held in reserve for *given* I' 's, but once committed, would symbolize only given I' 's, except that—and lastly—a relatively high pro-

portion of the N_k units, although committed to given I 's, could, under certain circumstances, be committed instead to other I 's. It is noted that a type of permanent "forgetting" is postulated. The essence of the approach, as contrasted with Condition B above, is that a relatively high proportion of the apparatus units would not be committed in advance to given I 's, i.e., that proportion of N_k held in reserve would be smaller than in Condition B. The approach is, obviously, a modification of Condition B.

Any suggested design of a system with the characteristics just indicated must fall into the realm of speculation, and therefore only the briefest suggestions will be made in the present paper. Let any I' be uniquely described by the subscripts 1, 2, 3, 4 ..., which indicate various hypothetical dimensions. Let k'_a be defined as k'_{1234} and k'_b as k'_{2345} . Call these 1st order units, i.e., $k'_{1(1234)}$ and $k'_{1(2345)}$. Postulate 2nd order units, as $k'_{2(-456)}$ and $k'_{2(-567)}$, in which the first indicated hypothetical dimension is not previously determined, but may be pre-empted by either of the 1st order units, depending upon which one occurs first. For example, if k'_a is committed before k'_b then the 2nd order reserve units indicated above would be $k'_{2(1456)}$ and $k'_{2(1567)}$ respectively. Further order units may be postulated, and it may also be postulated that pre-emption of 2nd order units by 1st order units, etc., does not persist indefinitely unless the lower order k' reserve is actually committed, and even then does not persist indefinitely, unless occasionally reactivated, provided that different higher order units are activated. The example, of course, is intended only as a paradigm, but consideration of it will reveal that, in addition to being relatively efficient in the sense considered earlier, it has

characteristics which might be involved in such phenomena as interference in learning, increasing difficulty in learning new material with increasing age, retroactive inhibition, transfer, and the general implications of "apperceptive mass." It is to be emphasized that, in any case, *it is with phenomena of this kind that an adequate theory of the psychoneurology of memory must cope.*

It is of course not necessary to assume that memorial units are in any direct way like the transient units from which they are derived, i.e., there is not necessarily a direct correspondence between I 's and I 's. It is conceivable that memorial units may represent selected characteristics of transient units, may symbolize common features of a sequence of such units, or relations or differences between them. The fact that ablation of local brain areas does not appear necessarily to affect memory is not conclusive evidence against each retention unit's being represented by a very few synapses: such evidence merely indicates that memory is not necessarily localized according to the dimensions of experience, which is transient in nature. Further, the efficiency of a memorial system in which only a very few synapses are required for each I' would be such that nearly equivalent memorial units could have multiple representation. It is also noted that while obviously many transient data are dependent upon memorial data, our argument does not require that moments be merely a direct translation of memorial units, i.e., it does not require that I 's be directly correspondent to I 's.

In conclusion it appears reasonable to postulate that transient data are dealt with in the CNS by a method generally similar to Condition A, and that memorial data are dealt with by

some method not similar to Condition A. This tentative generalization leads to two final implications:

1) Different neurons would be involved in transient and memorial functions. It is probable, though not necessary, that these different systems would have separate locations, though both systems might be widely distributed anatomically.

2) If the assumption of two basic neural systems is valid, then some kind of neural apparatus is required for translating the data of one into the data of the other. Suggestions regarding the details of such a mechanism will not be hazarded at this time.

To summarize: We have described two fundamental problems in psychoneurology as concerned with transient functions and with memorial functions, and have suggested that for transient processes the most efficient neuronal mechanism would be a binary system in which the number of unique expressions of information which could be represented equals 2 to

the power indicated by the number of included apparatus units; and further that for memorial processes some other system would be most efficient. Implications of these assumptions and relations between the two problems have been discussed.

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PSYCHOLOGICAL SCALING WITHOUT A UNIT OF MEASUREMENT

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I. INTRODUCTION

The concept of measurement has generally meant the assignment of numbers to objects with the condition that these numbers obey the rules of arithmetic (1). This concept of measurement requires a ratio scale—one with a non-arbitrary origin of zero and a constant unit of measurement (3). The scales which are most widely made use of in psychology are regarded as interval scales in that the origin is recognized to be arbitrary and the unit of measurement is assumed to be constant. But this type of scale should be used only if it can be experimentally demonstrated by manipulation of the objects that the numbers assigned to the objects obey the laws of addition. The unit of measurement in psychology, however, is obtained by a combination of definitions and assumptions, which, if regarded as a first approximation and associated with a statistical theory of error, serves many practical purposes. But

because we may sometimes question the meaning of the definitions and the validity of the assumptions which lead to a unit of measurement, it is our intent in this paper to develop a new type of scale not involving a unit of measurement. This type of scale is an addition to the types set up by S. S. Stevens (3). Stevens recognized ratio, interval, ordinal, and nominal scales. The type which we shall develop falls logically between an interval scale and an ordinal scale. We shall make no assumption of equality of intervals, or any other assumption which leads to a unit of measurement. We shall find, however, that on the basis of tolerable assumptions and with appropriate technique we are able to *order* the magnitude of the intervals between objects. We have called such a scale an "ordered metric." We shall develop the concepts first in an abstract manner with a hypothetical experiment and then illustrate the ideas with an actual experiment. Under the limitations of a single paper we shall not present the psychological theory underlying some of the concepts and we shall place certain very limiting conditions on our hypothetical data in order to simplify the presentation.

II. THE PROBLEM

When we set up an attitude scale by any of a variety of methods, for example the method of paired comparisons and the law of comparative judgment, we order statements of opinion on the attitude continuum and assign a number to each statement. We recognize in this instance

¹ This paper is a condensation of some of the ideas contained in a chapter of a general theory of psychological scaling developed in 1948-49 under the auspices of the Rand Corporation and while in residence in the Department and the Laboratory of Social Relations, Harvard University. While the author carries the responsibility for the ideas contained herein, their development would not have been possible without the criticism and stimulation of Samuel A. Stouffer, C. Frederick Mosteller, Paul Lazarsfeld, and Benjamin W. White in a joint seminar during that year. Development of the theory before and after the sojourn at Harvard was made possible by the support of the Bureau of Psychological Services, Institute for Human Adjustment, Horace H. Rackham School of Graduate Studies, University of Michigan.

that the origin for the numbers is arbitrary. We then follow one of several possible procedures (determining which statements an individual will indorse, for example) to locate the positions of individuals on this same continuum. Because both individuals and stimuli have positions on this continuum we shall call it a joint distribution, joint continuum, or J scale. In general, with a psychological continuum, we might expect that for one individual the statements of opinion, or stimuli, have different scale positions than for another individual. Thurstone (4) has provided the concept of stimulus dispersion to describe this variability of the scale positions on a psychological continuum. We have recently (2) discussed an equivalent concept for the variability of scale positions which an individual may assume in responding to a group of stimuli. These two concepts have been basic to the development of a general theory of scaling to which this paper is an introduction.

For didactic purposes we shall achieve brevity and simplicity for the presentation of the basic ideas underlying an ordered metric scale if we impose certain extreme limiting conditions on the variability of the positions of stimuli and individuals on the continuum. These conditions are that the dispersions of both stimuli and individuals be zero. In other words these conditions are that each stimulus has one and only one scale position for all individuals and that each individual has one and only one scale position for all stimuli. For purposes of future generalization we shall classify these conditions as Class 1 conditions. Stimuli will be designated by the subscript j and the position of a stimulus will be designated the Q_j value of the stimulus on the continuum. The position of an indi-

vidual on the continuum will be designated the C_i value of an individual i .

If we conceive of the attribute as being an attitude continuum, the C_i value of an individual is the Q_j value of that statement of opinion which perfectly represents the attitude of that individual. In this case the C_i value of an individual is his ideal or norm. We shall assume that the degree to which a stimulus represents an individual's ideal value is dependent upon the nearness of the Q_j value of the stimulus to the C_i value of the individual.

We shall then make the further assumption that if we ask an individual which of two statements he prefers to indorse he will indorse that statement the position of which is nearer to his own position on the continuum.

Thus if asked to choose between two stimuli j and k , the individual will make the response

$$j > k \quad (1)$$

if

$$|Q_j - C_i| < |Q_k - C_i|$$

where $j > k$ signifies the judgment "stimulus j preferred to stimulus k ."

Under the extreme limiting conditions we have imposed on the C_i and Q_j values the method of paired comparisons would yield an internally consistent (transitive) set of judgments from each individual, though not necessarily the same for each, and each different set of such judgments could be represented by a unique rank order for the stimuli for that individual. We shall call the rank order of the stimuli for a particular individual a qualitative I scale, or, in general, an I scale.

Thus if an individual placed four stimuli in the rank order A B C D as representing the descending order in which he would indorse them, then,

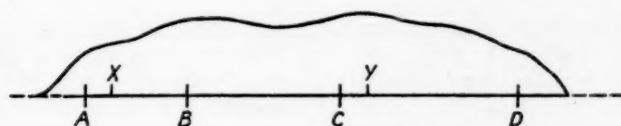


FIG. 1. A joint distribution of stimuli and individuals.

this would be equivalent to the consistent set of judgments $A > B$, $A > C$, $A > D$, $B > C$, $B > D$, $C > D$; where the symbol " $>$ " signifies "prefer to indorse," as before. The order, $A B C D$, is the qualitative I scale of this individual. Hence for Class 1 conditions it is sufficient to collect the data by the method of rank order; the greater power of the method of paired comparisons would be unnecessary and wasted.

Let us assume now that we have asked each of a group of individuals to place a set of stimuli in rank order with respect to the relative degree to which he would prefer to indorse them. Our understanding of the results that would follow will be clearer if we build a mechanical model which has the appropriate properties. This is very simply done by imagining a hinge located on the J scale at the C_i value of the individual and folding the left side of the J scale over and merging it with the right side. The stimuli on the two sides of the individual will mesh in such a way that the quantity $|C_i - Q_j|$ will be in progressively ascending magnitude from left to right. The order of the stimuli on the folded J scale is the I scale for the individual whose C_i value coincides with the hinge.

It is immediately apparent that there will be classes of individuals whose I scales will be qualitatively identical as to *order* of the stimuli and that these classes will be bounded by the midpoints between pairs of stimuli on the J scale. For example, suppose that there are four stimuli, $A B C D$, whose Q_j values or positions on a joint continuum are as shown in Fig. 1 and that there is a distribution function of the positions of individuals on this same continuum as indicated.

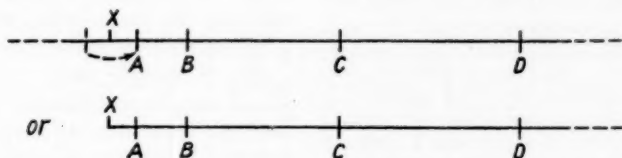
If we take the individual whose position is at X in Fig. 1, the I scale for that individual is obtained by folding the J scale at that point and we have the scale shown in Fig. 2.

The qualitative I scale for the individual at X is $A B C D$.

If we take the individual in position Y as shown in Fig. 1 and construct his I scale, we have the scale shown in Fig. 3.

The qualitative I scale for the individual at Y is $C D B A$.

Consider all individuals to the left of position X on the J scale in Fig. 1. The I scales of all such individuals will be quantitatively different for different positions to the left of X . For every one of them, however, the *order* of the stimuli on the I scale will be the same, $A B C D$. We shall re-

FIG. 2. The I scale of an individual located at X in the joint distribution.

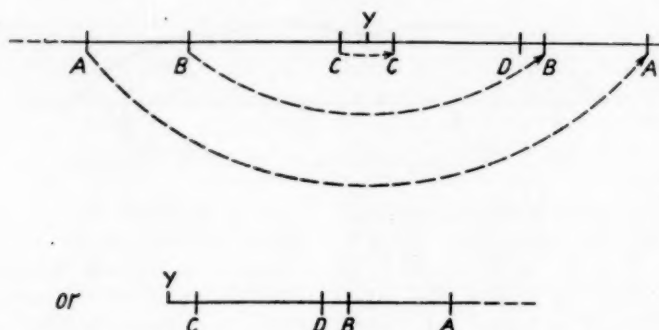


FIG. 3. The I scale of an individual located at Y in the joint distribution.

gard these I scales as being qualitatively the same. As a matter of fact, the I scales of individuals to the right of X continue to be qualitatively the same until we reach the midpoint between stimuli A and B. For an individual immediately to the right of this midpoint the qualitative I scale will be B A C D. I scales immediately to the right of the midpoint AB will continue to be qualitatively the same, B A C D, until we reach the midpoint between stimuli A and C. Immediately past this midpoint the qualitative I scale is B C A D. Continuing beyond this point a complicating factor enters in which we shall discuss in a later section under metric effects.

The distinction which has been made here between quantitative and qualitative I scales is of fundamental importance to the theory of psychological scaling. In almost all existing experimental methods in psychological scaling we do not measure the magnitudes $|C_i - Q_j|$, but only observe their ordinal relations for a fixed C_i .²

² The ordinal relations of $|C_i - C_j|$ may also be obtained experimentally for a fixed Q_j , over a set of C_i . We call such scales S scales by analogy with I scales. For sake of simplicity they are not treated here but actually the treatment for I scales and S scales is identical if the roles of stimuli and individuals are merely interchanged.

The kind of information which is obtained by the experimenter is essentially qualitative in nature.

As we shall see, data in the form of I scales may tell us certain things:

- 1) whether there is a latent attribute underlying the preferences or judgments,
- 2) the order of the stimuli on the joint continuum,
- 3) something about the relative magnitudes of the distances between stimuli,
- 4) the intervals which individuals are placed in and the order of the intervals on the continuum, and
- 5) something about the relative magnitudes of these intervals.

III. A HYPOTHETICAL EXAMPLE

Let us now conduct a hypothetical experiment designed merely to illustrate the technique. Of course this experiment, if actually conducted, would not turn out as we shall construct it, because we shall assume the extreme limiting conditions on Q and C values that were previously imposed.

Let us imagine that we have a number of members of a political party and that we have four individuals who are potential presidential candidates. Let us ask each member of the party to

place the four candidates, designated A, B, C, and D, in the rank order in which he would prefer them as President. With four stimuli the potential number of qualitatively different rank orders is 24—the number of permutations of four things taken four at a time. If there were no systematic forces at work among the party members we would get a distribution of occurrences of these 24 I scales which could be fitted by a Poisson or Binomial distribution, everything could be attributed to chance, and the experiment would stop there. Instead, let us imagine for illustrative purposes a different result equally extreme in the opposite direction. Let us imagine that from the N individuals doing the judging only seven qualitatively different I scales were obtained and these were the following:

I_1	A B C D
I_2	B A C D
I_3	B C A D
I_4	C B A D
I_5	C B D A
I_6	C D B A
I_7	D C B A

The significance of the deviation of such results as these from pure chance would be self-evident. Consequently we would look at these seven I scales to see if there was some systematic latent attribute represented by a joint continuum such that individual differences and stimulus differences on such a continuum could account for these manifest data. Studying the set of seven I scales we observe that two of them are identical except in reverse order, A B C D and D C B A. Fur-

thermore we see that in going from one to the next, two adjacent stimuli in the one have changed positions in the next. These are the characteristics of a set of I scales which have been generated from a single J scale. Seven I scales is the maximum number that one can obtain from a single quantitative J scale of four stimuli under the conditions of Class 1. The systematic latent attribute underlying this set of I scales is represented by the J scale which generates them. Our objective, then, is to recover this J scale and discover its properties or characteristics.

To recover the J scale we proceed as follows. Every complete set of I scales has two and only two scales which are identical except in reverse order. These are the I scales which arise from the first and last intervals of the J scale. Consequently these two I scales immediately define the ordinal relations of the stimuli on the J scale, in this case A B C D (the reverse order, of course, is equally acceptable). From the seven I scales we can order on the J scale the six midpoints between all possible pairs of stimuli. In going down the ordered list of I scales as previously determined, the pair of adjacent stimuli in one I scale which have changed places in the next I scale specify the midpoint on the J scale which has been passed.

Thus in the first interval (Fig. 4), we have all the individuals to the left of the midpoint between stimuli A and B. The second I scale is B A C D, and as stimuli A and B have changed places in going from the I_1 scale to the

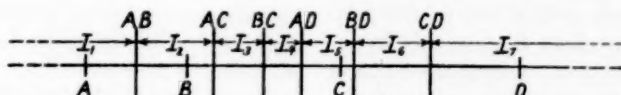


FIG. 4. An example of how the midpoints of four stimuli may section the joint distribution into seven intervals, each characterized by an I scale.

I_2 scale we have passed the midpoint AB. In going from I_2 to I_3 stimuli A and C exchange orders on the two I scales and hence the midpoint between A and C is the boundary between I_2 and I_3 . If we continue this process we see that the order of the six midpoints is as follows: AB, AC, BC, AD, BD, CD. These six boundaries section the joint distribution into seven intervals which are ordered as also are the stimuli. From the order of the six midpoints in the case of four stimuli we have one and only one piece of information about metric relations on the joint continuum. Because midpoint BC precedes AD we know that the distance between stimuli C and D is greater than the distance between stimuli A and B. We shall discuss these points and characteristics in more detail in the section on metric effects. There are then an infinite variety of quantitatively different J scales which would yield this same set of seven I scales—but there is only one qualitative J scale. The J scale in Fig. 4 meets the conditions necessary to yield the manifest data.

It must be emphasized that all *metric* magnitudes in Fig. 4 are arbitrary except that the distance from stimulus A to B is less than the distance from stimulus C to D.

With the qualitative information obtained in this experiment about the latent attribute underlying the preferences for presidential candidates the next task is the identification of this attribute. Here all the experimenter can do is to ask himself what it is that these stimuli have and the individuals have to these different degrees as indicated by their ordinal and metric relations. One might find in this hypothetical case that it appears to be a continuum of liberalism, for example, or of isolationism. One would then have to conduct an independent ex-

periment with other criteria to validate one's interpretation.

IV. METRIC EFFECTS

While the data with which we deal in the vast majority of scaling experiments are qualitative and non-numerical there are certain relations between the manifest data and the metric relations of the continuum. These relations have not all been worked out and general expressions have yet to be developed. The complexity of the relations very rapidly increases with the number of stimuli; therefore to illustrate the effect of metric we shall take the simplest case in which its effect is made apparent, the case of four stimuli.

With four stimuli, A, B, C, and D, there are 24 permutations possible. Thus it is possible to find 24 qualitatively different I scales. Also, obviously, each of these 24 orders could occur as a J scale which could give rise to a set of I scales. Half of the J scales may be regarded as merely mirror images of the other half. Thus if we have a J scale with the stimuli ordered B A D C and identify the continuum as liberalism-conservatism, then, in principle, we also have the J scale with the stimuli ordered C D A B and would identify it as conservatism-liberalism. Hence there are only twelve J scales which may be regarded as qualitatively distinguishable on the basis of the order of the four stimuli. I scales which are mirror images of each other, though, are definitely not to be confused. They may well represent entirely different psychological meanings. The direction of an I scale is defined experimentally—the direction of a J scale is a matter of choice.

Each J scale of four stimuli gives rise to a set of seven qualitative I scales. We are interested in know-

ing, of course, whether the J scale deduced from a set of I scales obtained in an experiment is qualitatively unique. The answer to this appears to be yes and immediately obvious when it is recognized that a set of I scales generated from a J scale has two and only two I scales which are mirror images of each other; that these two I scales must have been generated from the intervals on the opposite ends of the J scale, and that the order of the I scales within a set is unique. These statements are still to be developed as formal mathematical proofs and hence must be regarded as tentative conclusions.

However, a given qualitative J scale does not give a unique *set* of I scales. For example, with four stimuli, we may have the qualitative J scale A B C D. This order of four stimuli on a J scale can yield *two different sets* of I scales as follows:

set 1		set 2
A B C D		A B C D
B A C D		B A C D
B C A D		B C A D
B C D A	—	C B A D
C B D A		C B D A
C D B A		C D B A
D C B A		D C B A

It will be noticed that these two sets of seven I scales from the same qualitative J scale are identical except for the I scale from the middle interval. This arises from the following fact. There are six midpoints for the four stimuli on the J scale. These are as follows: AB, AC, AD, BC, BD, CD. The order and identity of the first two and the last two are immutable; they must be, in order: AB, AC, , BD, CD. But the order of the remaining two midpoints is not defined by the qualitative J scale but by its quantitative characteristics. If the interval between stimuli A and B is greater than the interval between C and D,

then the midpoint AD comes before the midpoint BC and the set of seven I scales will be set 1 listed above. If the quantitative relations on the J scale are the reverse and the midpoint BC comes before AD, then the set of seven I scales which will result are those listed in set 2 above.

Thus we see that in the case of four stimuli, a set of I scales will uniquely determine a qualitative J scale and will provide one piece of information about the metric relations. For five or more stimuli the number of pieces of information about metric relations exceeds the minimum number that are needed for ordering the successive intervals. *However*, the particular pieces of information that are obtained might not be the appropriate ones for doing this. It is interesting to note here that this is a new type of scale not discussed by Stevens. This is a type of scale that falls between what he calls ordinal scales and interval scales. In ordinal scales nothing is known about the intervals. In interval scales the intervals are equal. In this scale, which we call an ordered metric, the intervals are not equal but they may be ordered in magnitude.

As the number of stimuli increases, the variety of different *sets* of I scales from a single qualitative J scale increases rapidly. This means that a great deal of information is being given about metric relations. For example a J scale of five stimuli yields a set of eleven I scales (in general n stimuli will provide $\binom{n}{2} + 1$ different I scales from one J scale). Depending on the relative magnitudes of the four intervals between the five stimuli on the J scale, the same qualitative J scale may yield *twelve different sets* of I scales. This means that for a given

order of five stimuli on a J scale there are twelve experimentally differentiable quantitative J scales. Previously, in the case of four stimuli, we found only two differentiable quantitative J scales for a given qualitative J scale.

The particular set of I scales obtained from five stimuli *may* provide up to five of the independent relations between pairs or intervals. For example, suppose we have the qualitative J scale A B C D E. Among the twelve possible sets of I scales which could arise are the following two, chosen at random:

set 1	set 2
A B C D E	A B C D E
B A C D E	B A C D E
B C A D E	B C A D E
B C D A E	B C D A E
C B D A E	B C D E A
C D B A E	C B D E A
D C B A E	C D B E A
D C B E A	D C B E A
D C E B A	D C E B A
D E C B A	D E C B A
E D C B A	E D C B A

Let us see what information is given by each of these sets about the relative magnitudes of the intervals between the stimuli on the J scales. Consider set 1 first. The order of the ten midpoints of the five stimuli according to set 1 is as follows: AB, AC, AD, BC, BD, CD, AE, BE, CE, DE. We know immediately, from the fact that the midpoint BC comes after AD, that the interval between stimuli A and B (\overline{AB}) is greater than the interval be-

tween the stimuli C and D (\overline{CD}). We have summarized this in the first row of the table below. The other rows contain the other metric relations which can be deduced from set 1.

Or, in brief form, the I scales contained in set 1 indicate that the following relations must hold between stimuli on the J scale.

$$\overline{CD} < \overline{AB} < \overline{DE} \\ \overline{AB} + \overline{BC} < \overline{DE}$$

In the same manner we may study the implications of set 2 for the metric relations between stimuli on the J scale. The midpoints for this set are in the following order: AB AC AD, AE, BC, BD CD, BE, CE, DE.

SET 2

Order of midpoints	Relative magnitude of intervals on J scale
AD, BC	$\overline{CD} < \overline{AB}$
CD, BE	$\overline{BC} < \overline{DE}$
AE, BD	$\overline{DE} < \overline{AB}$
AE, BC	$\overline{CE} < \overline{AB}$

Or, in brief, the relative magnitudes of the intervals between stimuli on the J scale are known to the following extent.

$$\overline{BC} < \overline{DE} < \overline{AB} \\ \overline{DE} + \overline{CD} < \overline{AB}$$

The different implications of these two sets of I scales for the metric relations on the J scale may be illustrated by sketching two quantitative J scales which have the appropriate metric relations (Fig. 5).

The two sets of I scales which are illustrated here were only two of twelve possible different sets which could be generated from a single qualitative J scale of five stimuli. Each of the twelve sets of I scales would imply a different set of quantitative relations

SET 1

Order of midpoints	Relative magnitude of intervals on J scale
AD, BC	$\overline{CD} < \overline{AB}$
CD, BE	$\overline{BC} < \overline{DE}$
BD, AE	$\overline{AB} < \overline{DE}$
CD, AE	$\overline{AC} < \overline{DE}$

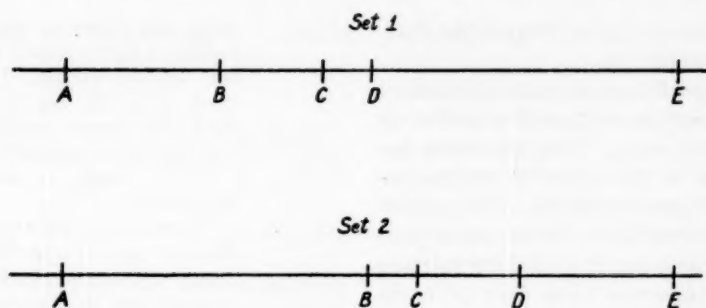


FIG. 5. An example of two joint distributions with the same order of stimuli but different metric relations obtained from different sets of I scales.

among the distances between stimuli on the J scale. The two sets of I scales used here happened to differ from each other in three of their particular members. If we take the twelve potential sets and make a frequency distribution of the number of pairs of sets which have 1, 2, 3, 4, or 5 particular I scales different or 10, 9, 8, 7, or 6 I scales in common, we get the following distribution.

Number of identical ordinal positions in a pair of sets with the same qualitative I scale	Number of such pairs of sets
10	18
9	24
8	17
7	6
6	1
$66 = \binom{12}{2}$	

The surface has not even been scratched on the generalizations which can be developed. Enough has been presented here to provide a general idea of the type of information which can be derived.

V. AN EXPERIMENT

In order to study the feasibility of this unfolding technique and to com-

pare several different psychological scaling techniques an experiment was conducted in several classes in the Department of Social Relations at Harvard University. A questionnaire was administered pertaining to grade expectations in a course. Data were collected suitable for four different kinds of analyses on the same content area from the same individuals. The four types of analyses for which data were collected were (1) the generation of the joint continuum by the unfolding of I scales obtained by the method of rank order, (2) the generation of the joint continuum by the unfolding of I scales obtained by the method of paired comparisons, (3) the generation of the joint continuum by what we shall call the Guttman triangular analysis, and (4) the generation of the joint continuum by what we shall call the parallelogram analysis.

We shall present here the analysis of only the rank order data. The experiment was first conducted in a graduate course in statistics and then, with a slight change in the wording of some questions, in an undergraduate course in sociology. Despite these differences in subjects and questions the general results were practically identical in the two groups and because our primary interest is in the technique and not the content of this

experiment we have lumped the data of the two groups.

The questionnaire was arranged as a small booklet with each question on a different page. The questions appropriate to the different techniques were deliberately mixed. The nature of the instructions, the separate paging for the questions, and the mixture of the questions were part of a deliberate effort to induce inconsistency, or at least to minimize a deliberate and artificial consistency. We felt this was accomplished but the high degree of consistency was surprising.

The content of the questionnaire follows.

page 1. *Instructions:* This is an experiment to test certain theoretical aspects of psychological scaling techniques. It is entirely voluntary and you need not answer the questionnaire if you so choose. However, you, as an individual, will not be identified; complete anonymity is preserved. We are interested *only* in certain internal relations in the data. This will become obvious to you because it will appear that we are getting the same information repeatedly in different ways.

You are free, of course, to mark these items entirely at random. It is our hope, however, that enough of you will take a serious attitude toward the experiment and make an effort to respond to each item on the basis of considered judgment.

The questions pertain to your grade expectations in this course. Of course, everyone wants an A or B, but we would like to ask you to give serious consideration to what you

really can expect to get. We want you to be neither modest nor self-protective. If you think you will get an A or flunk the course, make your judgments accordingly. Remember: there is complete anonymity.

There is one item or question on each of the following pages. Consider each question or item independently of the others. Answer each one without looking back at previous answers. Treat each item as an item in its own right and do not concern yourself with trying to be logical or consistent. Work quickly.

- page 2. In the following list of grades circle the two grades which best represent what you expect to get in this course.
A B C D E
- page 3. I expect to get a grade at least as good as a B.
yes no
- page 4. Of these two grades, which is nearer the grade you expect to get?
A D
- page 5. Of these two grades, which is nearer the grade you expect to get?
B C
- page 6. Of these two grades, which is nearer the grade you expect to get?
A B
- page 7. I expect to get a grade at least as good as a D.
yes no
- page 8. Of these two grades, which is nearer the grade you expect to get?
D E
- page 9. Of these two grades, which is nearer the grade you expect to get?
C A
- page 10. I expect to get a grade at least as good as an A.
yes no

- page 11. Of these two grades, which is nearer the grade you expect to get?
B E
- page 12. Of these two grades, which is nearer the grade you expect to get?
E C
- page 13. I expect to get a good grade.
yes no
- page 14. Of these two grades, which is nearer the grade you expect to get?
D B
- page 15. I expect to get a grade at least as good as a C.
yes no
- page 16. Of these two grades, which is nearer the grade you expect to get?
E A
- page 17. Of these two grades, which is nearer the grade you expect to get?
C D
- page 18. Place the five grades in rank order below such that the one on the left is the grade you most expect to get, then in the next space is the grade you next most expect to get, and so on, until finally at the right is the grade you least expect to get.
- | | | | | |
|-----------|---|---|-----------|---|
| — | — | — | — | — |
| 1 | 2 | 3 | 4 | 5 |
| the grade | | | the grade | |
| I most | | | I least | |
| expect to | | | expect to | |
| get | | | get | |

Page 18 of the questionnaire contained the rank order data. The total number of subjects for whom usable rank data were obtained was 121 (statistics class 40, sociology class 81). The individuals not included in the data which follow were people who introduced new grades (F), plus or minus grades, or left blanks. All individuals who wrote down the five letters A B C D E in some order are

contained in the analysis. The I scales obtained by the method of rank order and the number of people in each of the classes who so responded are given in Table I.

TABLE I

NUMBER OF PEOPLE IN TWO CLASSES
GIVING EACH OF THE RANK ORDER
I SCALES

I scale	Statistics class	Sociology class
A B C D E	14	6
B A C D E	10	22
B C A D E	6	21
C B A D E	1	4
C B D A E	1	11
C B D E A	2	3
C D B E A	1	0
D E C B A	1	0
B C D A E	3	7
B C D E A	0	6
B A C E D	0	1
C A B D E	1	0
Total	40	81

Let us first consider the two I scales in the bottom two rows of Table I. These two scales, B A C E D and C A B D E, were each given by one individual. There is no way that either of these two scales could have arisen from a J scale on which the stimuli are in the order A B C D E regardless of the metric relations on the J scale. All the evidence, both *a priori* and from the other response patterns, indicates that the order of stimuli on the J continuum is A B C D E. So we must regard these two I scales as errors on the part of respondents or assume some esoteric psychologies to explain them, the latter completely unjustified. Hence we shall drop these two individuals from further consideration.

Let us now look at the first eight scales listed in Table 1. From five stimuli we can have eleven different I scales to correspond to the eleven intervals into which the J scale is sec-

tioned by the ten midpoints between stimuli. Consequently these eight I scales constitute a *partial* set. But because one of the missing intervals (interval 8) has two alternative I scales, there are two possibilities for the complete set of which these eight are a partial set, as follows:

I scale	Total N
A B C D E	20
B A C D E	32
B C A D E	27
C B A D E	5
C B D A E	12
C B D E A	5
C D B E A	1
D C B E A — C D E B A	0
D C E B A	0
D E C B A	1
E D C B A	0

There were three intervals toward the low end of the J scale which were not occupied by any students. Apparently not very many students expected to get a low grade. The fact that one of the intervals on the J scale (interval 8) is blank and could be represented by two alternative I scales means that one of the metric characteristics of the intervals between stimuli on the J scale is not experimentally given. But from the remaining I scales the order on the J scale and some of the metric effects are determined.

The indications are that the order of stimuli on the joint continuum is A B C D E. We know this, of course, from *a priori* grounds, but the point is that this fact need not be known beforehand. Secondly, the individuals are placed in intervals and the intervals ordered on the continuum. The number of ordered intervals is eleven but individuals occupy only eight of the eleven intervals. Thirdly, we know certain metric relations among the intervals between stimuli. These are obtained as follows. From the order of the I scales within the set,

the successive midpoints between stimuli are: AB, AC, BC, AD, AE, BD, $\left(\frac{DC}{BE}\right)$, $\left(\frac{BE}{DC}\right)$, CE, DE. Because interval 8 was unoccupied and there are two alternative I scales which satisfy it, it is not known whether the midpoint DC or BE comes first. Hence one piece of metric information is lacking. In the table below are the metric relations which may be deduced and the basis for the deduction.

Order of midpoints	Metric relations
BC, AD	$\overline{AB} < \overline{CD}$
AE, BD	$\overline{DE} < \overline{AB}$

Or, in brief, $\overline{DE} < \overline{AB} < \overline{CD}$. The psychological distance between the grades D and E is the least and the distance between the grades C and D the largest, with the distance from A to B in between. No information is given of the relative magnitude of the distance between the grades B and C.

But now for another portion of the students there is a different interpretation. There are two I scales in Table I, B C D A E and B C D E A, which have not yet been considered. They are also members of a set that is as valid as the first set. If we remove I scales 4 and 5 from the partial set of 8 and substitute these two we have the following:

I scale	Total N
A B C D E	20
B A C D E	32
B C A D E	27
B C D A E	10
B C D E A	6
C B D E A	5
C D B E A	1
D C B E A — C D E B A	0
D C E B A	0
D E C B A	1
E D C B A	0

This set differs from the preceding only in I scales 4 and 5. If we analyze the significance of this set to the metric relations we have the following:

Order of midpoints	Metric relations
AE, BC	$\overline{AB} > \overline{CE}$

It appears from this that for the 16 individuals who gave the two I scales B C A D E and B C D E A, if they are treated as members of the same set, the psychological distance between the grades A and B is greater than either the distance from C to D or from D to E or, in fact, is greater than the sum of these two distances. This is in contrast to the 17 individuals in the first set who gave the I scales C B A D E and C B D A E in positions 4 and 5 for whom the relative distances between grades was in the order \overline{DE} , \overline{AB} , \overline{CD} .

The reader must be aware of the fact that it is only these 33 individuals for whom these metric relations are deduced. There is no way of knowing, for example, that the 20 individuals who gave the I scale A B C D E did so from the first interval on one of these two possible quantitative J scales or from any one of the many more differing in metric relations. An individual yielding the I scale A B C D E is known to be to the left of the midpoint AB and the relative distances \overline{BC} , \overline{CD} , and \overline{DE} do not affect his I scale. It is just the critical I scales which provide information about metric and this information is valid only for the individuals who yield these I scales. By putting them together we can construct a total picture, but our only evidence that they go together is one of internal consistency.

The general picture of metric relations among grades given by these two sets of I scales is the following:

- 1) For 17 individuals the metric relations are $\overline{DE} < \overline{AB} < \overline{CD}$
- 2) For 16 individuals the metric relations are $\overline{AB} > \overline{CE}$

Thus we find from the data that some of these individuals are simply on a different continuum from other individuals. To somehow compute scale values for the stimuli which will be assumed to hold for all these individuals is to do violence to the experimental evidence. From the data we have learned where an individual is on the continuum in relation to the stimuli, and in addition something about how the whole continuum looks to him.

VI. SUMMARY

We have presented a new type of scale called an ordered metric and have presented the experimental procedures required under certain limiting conditions to secure such a scale.

We have pointed out that the information which could be obtained under these conditions is as follows:

- 1) the discovery of a latent attribute underlying preferences,
- 2) the order of the stimuli on the attribute continuum,
- 3) something about the relative magnitudes of the distances between pairs of stimuli,
- 4) the sectioning of the continuum into intervals, the placing of people in these intervals, and the ordering of these intervals on this attribute continuum,
- 5) something about the relative magnitudes of these intervals.

These were illustrated with a hypothetical example and an experiment.

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COGNITIVE VERSUS STIMULUS-RESPONSE THEORIES OF LEARNING¹

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Contemporary learning theorists, whatever their predilections, seem to be in fair accord so far as the general statement of the problem or task confronting them is concerned. Learning psychologists, all seem to agree, are interested, first, in discovering and specifying the experimental variables that determine the observed behavioral changes that occur with practice, and, secondly, in the formulation of the functional interrelationships, *i.e.*, laws, holding between these sets of variables. Most learning theorists agree, furthermore, that this latter task seems to require the introduction and use of some type of theoretical construct.

It will be well at the outset to get clearly before us the problem confronting the learning psychologist. If we consider the behavior of an organism at any moment, the specific response made may be said to be a function of two sets of variables: (1) the particular world situation (physical or social) of the moment, and (2) the particular state or condition of the organism at the moment. The former is represented in Fig. 1 by S, and the latter by the oval.

Now the fact that in learning experiments the behavior or response of the organism to the same objective situation changes with successive practice has led the learning theorist to assume that certain changes must also occur within the organism. Unable to observe these

under-the-skin events, he has been led to speculate or theorize as to their nature. In the diagram I have represented these hypothetical learning changes by means of the symbol, L. As represented here, L, or the class of hypothetical learning factors, is assumed, on the one hand, to be the product of the past interactions of the individual with his environment and, on the other, to be one of the set of conditions or factors that determine his present performance. In this sense L is similar to constructs representing other hypothetical states of the organism that are assumed to be among the determinants of the behavior of the moment—*e.g.*, drive, fatigue, drug condition, etc.

Our diagram, then, attempts to show in a schematic way the relations between this hypothetical learning construct (L), other intervening theoretical constructs, such as the motivational (M), and the various experimental variables: (1) present environmental events (S), (2) past environmental events, or antecedent conditions (A.C.) and (3) the response measure (R).

Psychologists have differed widely in the manner in which they have conceived of these hypothetical learning changes, and it is in terms of these different theoretical conceptions that we can designate the main theoretical issues. Turning for the moment to psychological theorists whose main interests have not been in the problem of learning—*e.g.*, the late Professor Lewin—the favorite method has been to infer (define) them from behavior, particularly from the verbal introspections of either their subjects or themselves (16, 17).

¹ This paper represents the main address, with some modifications, given in a symposium on learning of the Division of Theoretical-Experimental Psychology at the Boston meetings of the American Psychological Association in 1948.

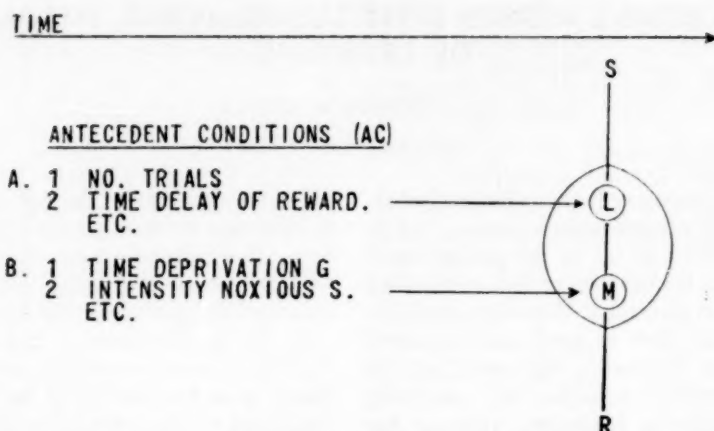


FIG. 1. Schematic representation of the experimental variables and theoretical constructs with which the learning psychologist is concerned.

In the case of Lewin, symbols or terms taken from entirely different disciplines—*e.g.*, topology and dynamics—have been employed to represent these introspectively or response-derived notions.

A second group of psychologists who have favored this method of inferring their hypothetical constructs from behavior is the Gestaltist. While primarily interested in problems of perception and the complex problems of thought and reasoning, Köhler (15) and Koffka (14)—particularly the latter—have given no little attention to the problem of learning. The extent to which phenomenological introspection has entered into the theories of these psychologists is well known.

There is a further characteristic of the Gestaltists' thinking to which attention may be directed at this time. In contrast to Lewin's topological and dynamic constructs, those of Köhler and Koffka have a neurophysiological ring. Thus they introduce such concepts as brain field, neurophysiological traces and trace systems, etc. This tendency to describe the hypothetical response-determining factors in terms of neurophysiological models is a very prevalent

one among psychologists, whether Gestalt or otherwise. Apparently it is akin to the physicists' construction of models representing the internal state of the atom. However, unlike the theoretical physicist who typically uses these models merely for expository purposes and who does his real theorizing in terms of mathematical constructs, the psychological theorist is more often likely to have only the model. In such instances one does not usually have any theory at all but only a simple analogy which more often than not explains nothing.

There is still another type of analogy which has been used all too frequently by some psychological writers. I refer to the practice of likening the hypothetical internal events within the organism to a telephone switchboard in which learning is represented as consisting in the alterations in the resistance or conductance of the various connecting switches. Such pictures should never, of course, have been taken as serious, responsible theorizing, but rather what they were most often meant to be—a device often employed by writers of elementary textbooks to try to convey something that even a college sopho-

more will believe he understands. Elementary textbook writers do not, at least nowadays, attempt to represent, in the best scientific manner possible, the various theoretical aspects of psychology. Everyone knows such books are written to sell, and to sell they apparently must use such simple, familiar analogies.

Unfortunately for the S-R point of view, many of the current elementary textbooks are written in terms of this schema and as a consequence its opponents have come to imply that the switchboard is a necessary part of the S-R position. Thus, in a recent number of this JOURNAL, Tolman (32) refers to the "stimulus-response" viewpoint as the "telephone-switchboard school of psychology." No scientifically oriented person in psychology, however, would ever take such analogies, whether telephone switchboards or map control rooms, as serious attempts at theoretical representation of learning changes.

This brings us to the consideration of the last of the techniques which have been employed for introducing or defining the hypothetical states or conditions in the organism supposed to determine behavior. I have reference here to the method of defining them as mathematical functions of the present stimulus and the antecedent environmental conditions. The best examples of this method are Hull's constructs of habit (sH_R) and drive (D) (10). Tolman's program for theorizing is essentially similar in plan (30, 31).

With this picture of the varieties of psychological theorizing before us, we now turn to the particular issue in hand—namely, cognitive versus stimulus-response interpretations of learning. This distinction is emphasized primarily by the cognitive group and, as we shall see, the psychologists falling into this group are united about as much by their op-

position to what they conceive the S-R position to be as by the positive notions they have to offer by way of an alternative.

I shall begin by presenting the positive side of the theoretical picture that the cognition, or S-S, theorists offer. The essential notion underlying the theorizing of all members of this group seems to me to be that learning is part of a larger problem of organization, including, as a most important aspect, *perceptual organization*. These theorists all agree that learning is to be conceived in terms of the organization into some kind of functional whole of the perceptual systems of the subject. Thus, in referring to the simple type of learning involved in the CR situation, Zener writes:

"... the essential structural modification consists in a reorganization into some kind of functional whole of the perceptual systems corresponding to the conditioned and unconditioned stimuli; and in the functional relation of this organized system to the urge or tension system originally excited by the unconditioned stimulus" (34, p. 386).

Adams (1), Koffka (14), Lewin (17) and Tolman (30, 32, 33) likewise emphasize that learning involves primarily the structuring (or restructuring) of the cognitive field of the subject—i.e., the formation and modification of cognitive patterns representative of the relationships in the environment.

The following statement, quoted from White's article, in which he attempted to clarify the Tolman-Lewin interpretation of learning, is, in my opinion, one of the best brief statements of the cognitive theory of learning: "The perceptual-learning postulate implies the importance of *perceptual 'field' conditions at the time of the original perception*, rather than any subsequent reward or 'reinforcement.' This difference is both

an affirmation and a denial. It affirms the importance, in relation to perception, of all those field conditions which have been experimentally shown to influence perceptual organization: temporal contiguity, spatial contiguity, visual continuity, common contrast, embeddedness, exploratory motivation, etc. All of these factors, except temporal contiguity, have been given less emphasis by *S-R* psychologists" (34, p. 166).

So much for the cognitive theory of learning. Let us turn now to a similar brief survey of the main concepts of the *S-R* theory. Instead of conceiving of learning in terms of perceptual or cognitive changes, the *S-R* learning theorists refer to such things as stimulus-response connections, bonds, associations, habits, or tendencies. As Hull (10) and Thorndike (28) have used these concepts, they have had reference to a hypothetical learning state or intervening variable. In effect Thorndike's original three major laws of learning (27) provided a definition of his concept of *S-R* bond in terms of the experimental conditions, variation of which would lead to changes in its strength. Later, in connection with his experimental studies with human subjects, Thorndike (28) introduced six further experimental variables that must, he believed, be taken into account in specifying the strength of an *S-R* bond or association. These variables are belongingness, impressiveness, polarity, identifiability, availability and mental systems (or set). It is rather interesting to note that most of these latter variables are of the type that the cognitive theorists, particularly the Gestaltists, emphasize. They refer to the content of the materials learned rather than to relations—*e.g.*, temporal—between the contents.

Thorndike's major interest has been in the identification of the experimental

conditions that are responsible for the occurrence of learning. In particular he has attempted to discover and define the necessary motivational-reward conditions. This task involves primarily the operational definition of a "trial" or "reinforcement" in a learning experiment. Thorndike has never particularly concerned himself with the problem of specifying the nature of the functional relationship between his hypothetical intervening variable, *S-R* bond, and these antecedent experimental variables.

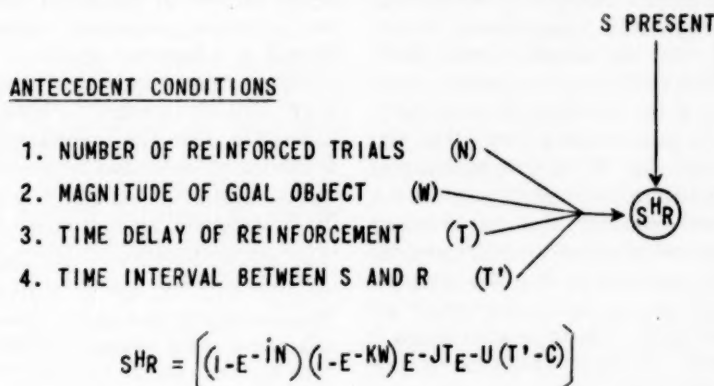
In sharp contrast to this relatively circumscribed interest of Thorndike, Hull (10) has been interested not only in the problem of specifying, operationally, the experimental variables determining his hypothetical learning factor—*habit*—but, also, he has attempted to guess at the "law" describing how these variables combine to determine *habit* strength (sH_R). Thus he postulates that *habit* is a specific function (shown in Fig. 2) of at least four variables: (1) the number of reinforcements (N); (2) magnitude of the reward (W); (3) time of delay of the reward (T); (4) time interval between the stimulus and response (T'). Hull then goes on to specify certain further hypothetical relations that *habit* has to other hypothesized intervening variables such as drive (D), excitatory potential (E), inhibition (I), etc. He finally ends his theorizing by relating these hypothetical intervening variables to the several empirical response measures used in simple conditioning experiments. Incidentally it is important to note that in his *Principles of Behavior* Hull has dealt almost exclusively with classical and instrumental conditioning. Because of the simplicity of these experimental situations he believes that they provide the best means of revealing the basic learning principles. It is his intention

to employ the basic constructs and hypothetical laws discovered there, plus whatever additional ones are necessary to explain more complex learning phenomena. Some anticipations of this program, so far as maze learning and certain other more complex learning situations are concerned, appeared in a series of articles in this JOURNAL (4, 5, 6, 7, 8, 10, 11) in the 1930's, and the present writer has attempted to extend Hull's principles to simple *T-E* learning (25), discrimination learning (21) and transposition phenomena (22).

simplified elementary textbook treatments.

It is therefore pertinent to elaborate and comment on some of these differences, supposed and real.

(1) First, it should, I believe, be clear that there is nothing that implies the notion of a telephone switchboard in Hull's *mathematical* specification of *habit*. The meaning of this construct is given by the mathematical function relating it to the antecedent experimental variables and the operational definition of the latter. Any comparison, then, of



HULL'S MATHEMATICAL SPECIFICATION OF HABIT STRENGTH $S^H R$

FIG. 2. Hull's mathematical specification of habit strength (sH_R).

What are the main points of disagreement between these two theoretical positions? My own reaction to this question is that the differences have been greatly exaggerated and, mainly, I fear, by the cognition theorists. Instead of making a serious attempt to understand the essential nature of Hull's and Thorndike's theorizing, the cognition theorists have either seized upon certain unfortunate, secondary and irrelevant features of their treatments, or have taken their conception of *S-R* theory from over-

switchboards with map control rooms is entirely beside the point so far as this theory is concerned.

(2) Secondly, with regard to the neurophysiological models favored by the two opposing camps, there undoubtedly is a considerable difference, but, again, I should like to emphasize that this difference has little or no significance so far as learning theory is concerned. Just why Hull (10), after formulating his mathematical theory of *habit*, found it necessary to elaborate a neurophysio-

logical model of receptor-effector connections, has always remained a puzzle to me. Actually he does little more than identify or coordinate this concept of receptor-effector connection with his mathematical construct of habit. I doubt whether a single one of the deductions with respect to learning in his *Principles of Behavior* would be lost or changed in any way if it were eliminated. These implications follow exclusively from his mathematical theorizing and not at all from the superfluous physiological model.² The same is true for Thorndike's theory about the alteration of synaptic conductances, and I suspect that the electrical brain fields that Köhler offers as isomorphic counterparts of his hypothetical trace fields add little more to his theory. The picturing of what the neurophysiological processes are *without specifying the hypothetical relations that tie them up with the experimental variables and the response measures* is almost a complete waste of time so far as furthering our understanding of learning phenomena is concerned. I suspect that I am in close agreement with Tolman on this point for he seems, at least so far, to have quite successfully escaped from the compulsion to engage in brain speculation.

(3) My third point is concerned with the problem as to whether the functional tie-up or association established in learning is between sensory and motor processes, or between sets of sensory processes. The cognition theorists appear to be quite united in the view that learning involves the association (they would prefer to say the organization) of sensory or perceptual

processes. Thus Zener's statement, quoted earlier, that conditioning involves the "reorganization into some kind of functional whole of the perceptual systems corresponding to the conditioned and unconditioned stimuli" will be recalled. Maier and Schneirla (19) likewise explicitly state that the essential change in conditioning involves a new dynamic relationship between the sensory cortical patterns of the conditioned and unconditioned stimuli. And Tolman's concept of sign-Gestalt-expectation may be thought of as a cognitive pattern in which are associated the successive perceptual processes occurring in a behavior sequence.

On the other hand, the S-R theorists have certainly tended to hold to the conception that the association is between the stimulus and response mechanisms. Thus in attempting to contrast his behavioristic associationism with the older associationistic doctrines, Guthrie wrote as follows:

"Our position is that what is associated is a stimulus and a response. It would perhaps be more exact to say that what is associated is some stimulation of sense organs and a corresponding muscular contraction or glandular secretion" (3, p. 43).

Thorndike and Hull have also definitely implied that the tie-up in learning is between receptor and effector mechanisms *although it is important to note that Hull's mathematical definition of habit does not identify it either as an S-S or S-R concept. It is only by virtue of his additional neurophysiological theorizing that Hull falls into the S-R group.* So far as I am concerned I do not find it difficult to conceive of both types of organizations or associations being established in learning. Certainly simple types of perceptual learning would appear possibly to involve intersensory associations. I seriously doubt, however,

² Since the above statement was written Hull has discussed his reasons for adding his neurophysiological speculations to his mathematical theorizing. See footnote 4 in his recent article on the gradient of reinforcement (11).

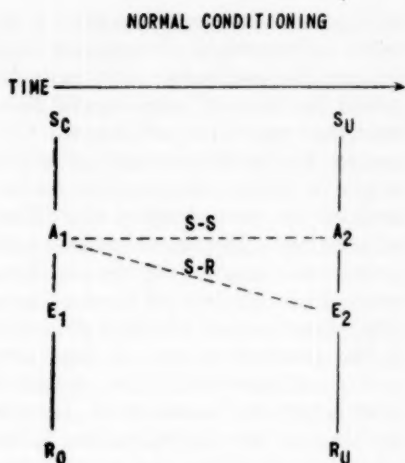


FIG. 3. Schematic representation of the classical conditioned response showing by means of the broken lines the two possible hypothetical associations (S-S or S-R).

whether learning is exclusively of this type, or even that the majority of it is. Indeed, what little experimental evidence there is on the point, even in the field of such simple learning as conditioning, would appear to support more

strongly the S-R conception than the S-S.

Figures 3 and 4 describe one such experiment. In Fig. 3 the typical classical conditioned response situation involving a defense response to shock is represented. The conditioned stimulus (S_c) is indicated as leading to an afferent process (A_1) and the unconditioned stimulus to afferent process (A_2). E_1 and E_2 represent the efferent neural processes that result in the original responses, R_o and R_u respectively, to S_c and S_u . The hypothetical association or organization occurring in conditioning may be indicated by either of the two dotted lines, depending upon which theory is held to—an S-S or S-R.

Loucks (18) has reported an experiment in which S_u and A_2 were eliminated, the unconditioned flexion response being elicited by applying faradic shock directly to the appropriate area of the motor cortex. This procedure, of course, by-passed the usual sensory system, pain from shock to limb, involved in normal conditioning (see part A, Fig.

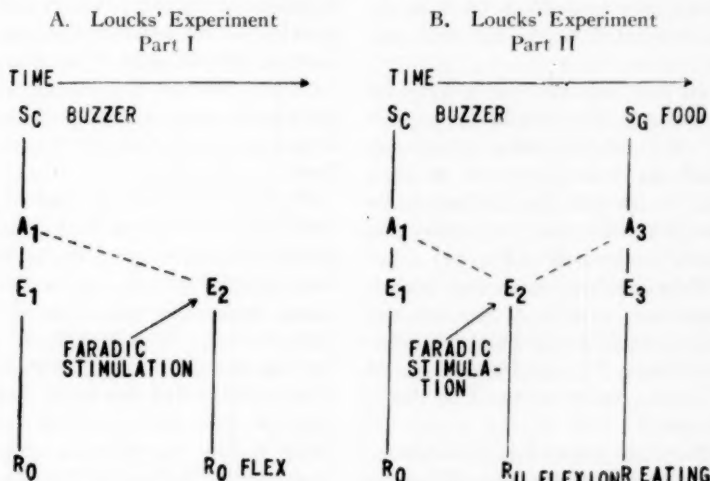


FIG. 4. Schematic representation of two parts of Loucks' conditioning experiment involving bypassing of the afferent system of the unconditioned response.

4). Hence the association between A_1 and A_2 could not take place, but the association between A_1 and E_2 was still theoretically possible. Loucks actually found that no conditioning took place under this condition and this result was cited by Maier and Schneirla (19) as evidence in favor of their S - S hypothesis and against the S - R conception.

But there was a second part to the Loucks experiment, as described in part B of Fig. 4. In this part of the experiment Loucks followed each leg flexion with the presentation of food, and conditioning did occur. Maier and Schneirla's attempt to explain this result away is most unsatisfactory. I quote them:

"Under these conditions foreleg flexion became associated with reward just as string pulling becomes associated with reward in the problem box situation" (19, p. 126).

In other words, they are saying, an association between E_2 and A_3 became established and is responsible for the conditioning. Just why this is not an S - R connection is not clear to me, nor why such a backward association would be formed, whereas association between A_1 and E_2 would not be, is also left unclarified.

The explanation of the two parts of this experiment that would be offered by the S - R theorist is probably obvious. He would say that there was no reinforcement in the first part of the experiment and hence the necessary condition for association-formation was not present. When reinforcement was added, the association between A_1 (the sensory mechanism) and E_2 (the effector mechanism) occurred. This explanation would not, of course, work for the S - R contingency theorist.

(4) The next point I should like to discuss presents a very real difference between the two theoretical camps, but the difference seems to me to be one of

emphasis rather than of conflict. I refer to the tendency of most cognition theorists to emphasize what may be termed the intrinsic properties of their theoretical constructs, whereas the S - R theorist has tended to emphasize the properties of his concepts that are determined by the antecedent experimental variables. The learning theories of the Gestalt members of the cognition group, in particular, are marked by a very extensive and detailed discussion of the inherent nature of their constructs. Thus Koffka (14) treats at great length the properties of the various kinds of hypothetical factors he assumes, *e.g.*, processes, traces, trace systems, ego systems, etc. While it is true that Koffka also mentions some experimental variables that he believes to be important in determining the properties of his hypothetical learning constructs, for the most part these have consisted of the conditions in the present stimulus situation which play an important part in the perceptual processes. The implication is usually given that analogous relationships (*i.e.*, laws) will be found to hold with respect to traces and, hence, learning. The belief on the part of the Gestalt psychologist that learning is merely a part of the larger problem of perceptual organization provides much of the rationale for this appeal to the laws of perception.

Probably another important factor determining the approach of the Gestaltists to the problem of learning theory is their decided preference for the mediational type of explanation of psychological events. For Köhler, in particular, explanation of psychological events is to be found in terms of the underlying neurophysiological processes. At every point Köhler attempts to indicate the isomorphic relations of his trace fields to electrical brain fields. Unfortunately the properties of these hypothetical

brain fields are more often inferred from phenomenological introspections than based on the experimental findings of brain physiology.

Once more I should point out that Tolman's sign-Gestalt formulation represents an exception to this tendency on the part of the cognition theorists to concern themselves extensively with the intrinsic properties of their theoretical constructs. The first psychologist to recognize the role of the intervening variable as an aid to the discovery and formulation of laws in psychology, Tolman has always insisted that these theoretical constructs must be defined in terms of the independent (environmental) variables. It seems to be characteristic of the behavioristic-oriented psychologists that they tend to direct their interests toward explanations that refer to events in the physical and social environment, past and present, rather than to events in the brain.

(5) The fifth point of difference between the two opposed schools of thought has reference to the different independent experimental variables emphasized in the work of the two groups. The cognition theorists have been much more interested in the conditions that determine the reception of the stimulus events and that influence perceptual organization. Thus they have concerned themselves extensively with the effect of such experimental variables as figure-ground conditions, set, visual continuity, embeddedness, belongingness, fusibility, etc., on learning. The S-R psychologists, on the other hand, have been most interested in the effect of various temporal factors, such as time interval between trials, and in the motivational-reward conditions underlying learning, etc. Again the differences with regard to these factors are relative, for the cognition theorists do not confine their studies exclusively to perceptual factors

and the S-R theorists have not been concerned only with reinforcement conditions.

Such differences of emphasis, it should be noted moreover, do not necessarily involve conflict. There still remains much to do before we have a reasonably complete understanding of even simple learning phenomena, let alone the more complex types of adjustment. Preoccupation with the reinforcement conditions of learning by the S-R psychologist does not mean, as some cognition theorists appear to have felt, that the former believe stimulus-reception factors are not important. It is merely a reflection of their greater interest in such matters. Similarly if the cognition theorist enjoys speculating about the physiological properties of his hypothetical learning factors, I do not feel that his work is in conflict with non-physiologically oriented attempts at theorizing. I do, of course, think that the latter procedure is more fruitful so far as learning phenomena are concerned *at the present time*.

(6) There are a number of other aspects of this problem of stimulus-reception that have been a source of much difficulty and misunderstanding between the two groups of theorists. Perhaps the best way of introducing the discussion of the next point is to paraphrase some of the statements concerning the matter made by Tolman in a recent article entitled, "Cognitive Maps in Rats and Men." Tolman writes: "According to the stimulus-response school, the subject in learning a maze responds helplessly and passively to the succession of external and internal stimuli" (32, p. 189). In contrast, he states that while his theory admits that the subject is bombarded by stimuli, he holds that the nervous system is highly selective as to which of these stimuli it will let in at any given time. In commenting on an

experiment of one of his students, Tolman writes further:

"... this experiment reinforces the notion of the largely active, selective character in the rat's building up of his cognitive map. He often has to look actively for significant stimuli in order to form his map and does not merely passively receive and react to all the stimuli which are physically present" (32, p. 201).

Presumably the last portion of this quotation again has reference to the S-R point of view.

It is difficult to know for sure just what Tolman and others (Krech, Lashley, etc.) who have expressed this same notion mean by this kind of statement but the point comes up so persistently it is time that S-R psychologists attempted to clarify their position. One possible reason for the belief expressed by Tolman is that much of the S-R theory is concerned with the classical conditioning situation in which the conditions of stimulation are extremely simple. No "active looking" for the cue-stimulus in the sense of trial and error receptor-orienting acts is necessary in this situation. In the case of human subjects a set to orient towards the stimulus, if it is visual, is provided for by preliminary verbal instructions. In the case of animals an auditory stimulus or change in the general illumination has generally been used. Receipt of either of these types of stimulus requires no preliminary learning of a special receptor orientation. The subject receives the stimulus regardless of what it is doing or how its receptor mechanisms may be oriented.

But even in the case of this simple learning situation the S-R theorist has not assumed, as claimed, that organisms passively receive and react to all the stimuli that are physically present. In Chapter III of his book, *Principles of*

Behavior, Hull distinguishes between the *potential stimuli* of a situation and the actual stimuli being received at any moment by the organism. In the thirteenth chapter he further discusses at some length a number of factors that determine the amount of habit loading acquired by different components of a stimulus compound. Thus he recognizes that such factors as static vs. changing stimulus, intensity, type of receptor, pervasiveness of the stimulus, etc., may lead to different habit loadings. Very little experimental evidence on these matters is available; hence Hull has not theorized so extensively or as specifically about them as he has about certain other factors.³

This misunderstanding of the S-R position with respect to stimulus reception has also been chiefly responsible for the controversy known as the continuity-non-continuity issue in animal discrimination learning. On the one side of this disagreement the cognition psychologists have interpreted the systematic responses that occur during the pre-solution period when the subject is responding chance to the cue stimuli as involving a selective concentration on certain other stimulus aspects as the result of "sensory-organization processes." They assume further that no learning or cognitive formation occurs with respect to the cue-differences during this period. Ultimately the animal responds "perceptually" to the cue-aspects and from then on, and then on only according to this view, does it form cognitions about these stimuli. The original experiments on which this interpretation arose and was tested involved discrimination of brightness (12) and weight (20) respectively,—stimulus dimensions, it should be observed, that

³ In order to reduce the length of the talk, the remainder of the paper was not presented in the original presentation.

require no learning of special receptor-orienting adjustments in order to be received. Subsequently a form discrimination situation (13) was employed by Krechevsky in which it was necessary, first, for the subjects to learn certain appropriate visual receptor orienting acts in order to provide for the reception of differential positive and negative cues. Ehrenfreund's recent experiment (2), based on the *S-R* interpretation of these phenomena, has shown clearly the role that such preliminary receptor orienting acts play in the learning of visual discrimination involving differences such as form.

Quite contrary to its opponents' claims, then, the *S-R* theory does not assume that the animal passively receives all the physically present stimuli. In more complex learning situations, such as discrimination, simple trial and error and maze learning, etc., the subject cannot possibly receive all of the visual stimulus situation at any one moment. More important still, any particular receptor exposure adjustment that the subject may have picked up in the preliminary training or other prior experiences may be such as not to provide discriminably different stimulation from the positive and negative stimulus-cues. Thus a triangle, the light rays from which strike the periphery of the retina, as the result of the particular fixation habits, will not be discriminated from a circle also in the periphery of vision. The early stages of learning situations more complex than classical conditioning involve, as an important part of them, the acquisition of these receptor exposure

adjustments that provide the relevant cue. Such learning is itself an active, trial-and-error process with those adjustments being learned that lead to reception of stimulus-cues, responses to which are followed by reinforcement.

My final comments also relate to an aspect of the problem of perception. Unlike the *S-S* theorist, the *S-R* psychologist does not usually talk very much about such things as perception, meaning, knowledge, cognitive processes, etc. I suspect, however, that he deals with pretty much the same things that the cognitive theorists do under different terms. For example, if asked to give an analysis of perception or cognition, the *S-R* psychologist with his analytical bent would probably proceed very much as follows. First of all he would be likely to distinguish a number of different aspects as shown in the diagram in Fig. 5. Time does not permit a detailed elaboration but I will indicate in a most schematic manner the essential nature of each aspect and some of the most pertinent problems that exist concerning them.

1. *Sense reception*: This refers to the activity of the receptor mechanisms including presumably their terminal activities in the brain. One problem here is the extent to which organization into units or patterns occurs, and if so, to what extent such organization is innate or learned. The Gestalt psychologists have placed heavy emphasis on the innate factors although other cognition theorists have given more recognition to the possibility of the role of learning in such processes. So far as *S-R* theorists

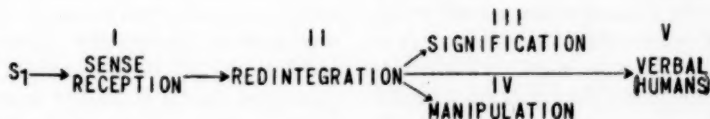


FIG. 5. An analysis of perception (cognition) from an *S-R* point of view.

are concerned, about the only treatment of this aspect of reception is Hull's construct of stimulus trace and his hypothesis of afferent interaction between these traces.

2. *Redintegration*: I have chosen this term to refer to the phenomenon that a particular sensory process arouses other sensory experiences, e.g., the sight of ice arouses experience of cold, etc. There is a suggestion that sensory processes that have been experienced contiguously in the past tend to become associated. These may involve associations between externally initiated sensory processes or between externally and internally aroused processes. One of the questions to be answered here is whether these associations or habits are between the sensory items as the cognitive theorist would insist or whether each sensory process arouses its particular efferent process and the associations are between afferent-efferent processes. This is the same question we dealt with before and I should like to repeat that so far as I am concerned I do not feel the need to speculate as to the neurophysiological basis of the habits formed. I prefer to confine my hypothesizing to certain quantitative properties of these habits that can be defined in terms of the experimental variables, environmental and behavioral.

3. *Signification*: Whereas redintegration referred to habit organizations or associations between temporally contiguous sensory processes, signification refers to associations between the stimulus-aroused events that in the past have occurred in temporal succession. The first stimulus event becomes, we say, a sign for the coming of the second event. Here, of course, the classical example is the Pavlovian-type conditioning situation, but whereas the cognition theorists would be likely again to insist on the association being between the successive

sensory aroused events, the S-R psychologist is more likely to assume afferent-efferent connections. As we have already seen, what little experimental evidence there is on this type of learning favors the latter interpretation (cf. Loucks' experiment).

Attention should be drawn here to the fact that the emotional significance of environmental events falls into this class of perceptual response.

4. *Manipulation*: The meaning of stimulus objects or events is provided not only by the kinds of internal responses (e.g., emotional, anticipatory goal responses, etc.) that are made to it, but also in terms of the overt responses that are made directly to it. Thus the meaning of "knife" or "ball" is given, in part, in terms of the manipulatory acts (e.g., cutting, throwing) that are learned with respect to them.

5. *Verbal meaning*: Finally, in the case of humans there are the meanings provided by learned verbal responses. The degree of sophistication that has been attained here, particularly in the abstract aspects of language, and the extent to which many of the behavior difficulties of human beings are the result of disorders of verbal behavior, are well known if not as yet well understood. They badly need the attention of all psychologists, cognition, S-R or whatever else.

In concluding this discussion I should not like to leave the impression that I believe the S-R psychologist has by any means adequately dealt with the problems of sense reception and perception in learning. His primary interests, as was indicated before, have been in other areas, i.e. with other variables. He has not, however, completely neglected the problem, and his point of view is not the naive one that it is usually represented to be in the writings of the cognitive theorists. Far too prevalent in the writ-

ings of current cognitive psychologists is the deplorable technique of misrepresenting the formulations of opposing viewpoints and then showing these erroneous formulations to be inadequate. One almost gets the impression that the major goal is to prove the other conception wrong rather than to try to arrive at a more comprehensive interpretation of the phenomena. Psychologists interested and appreciative of the role of theory in the development of a scientific body of knowledge should resist such temptations. The main result of such theoretical fencing is likely to be the bringing of theory into disrepute. Already many extremely empirical minded psychologists are thoroughly disgusted with the theoretical debates that go on in this field. Their proposed remedy, elimination of all theorizing, would certainly not help to speed up the acquisition of a scientific body of knowledge about learning.

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BEHAVIOR POSTULATES AND COROLLARIES—1949

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The matter of isolating and formulating a set of quantitative postulates or mathematical primary principles upon which may be based a true natural-science theory, one designed to mediate the deduction of a system as complex as that concerning mammalian behavior, is a formidable undertaking. To write out the blank form of an equation such as $x = f(y)$ is quite simple, but careful planning and a certain amount of skill are often required in the determination of what the function actually is and the approximate values of the numerical constants contained in it. Moreover, suitable units of quantification must be devised. There must ordinarily be added a preliminary checking of the consistency of the formulation with known empirical facts. All this takes much time and effort. The detailed history of such a process would itself require a fair-sized volume.

Very briefly, the present writer's procedure may be described somewhat as follows. He begins with selecting two or three principles, often isolated by earlier workers, from the complex of data involving a certain class of experiments. These are generalized and quantified as tentative equations. The attempt is then made to apply the equations to a wider range of phenomena. So long as the application of these equations agrees with empirical fact they are retained. But when it seems that a given combination of formulated principles ought to mediate the deduction of an empirically known phenomenon but does not do so, the principles in question are reexamined to the end that with the possible revision of one or more

of them they will yield the deduction sought and still mediate the true deductions already to the credit of the critical postulates.

Sometimes the deduction failure may appear to be the result of ignoring a law not yet formulated. An example of this in the present postulate set concerns the matter of stimulus-intensity dynamism (7), which appears as Postulate VI. In such a case an empirical situation is sought in the literature, where all the factors are held constant except the significant ones in question; the data which are found are plotted and an equation is fitted to these values. Usually such data are defective in one way or another, though if facilities are not available to set up a specific experiment they will be used as a first approximation. The provisional equation so secured is then tried out deductively in more general situations with other principles which presumably are also operating with it. Thus it may be seen that a large element of trial and error is involved in the process. It is to be expected that failures among such trials would be relatively more frequent in a young and fast-moving science than in an older and more stable one.

A detailed record of the theory trials made by the present writer in the behavior field is scattered through twenty-six volumes of handwritten notebooks. This series began in 1915 and extends to the present time. The system grew very slowly, especially during the early years. The first published material in the series appeared in 1929 (1). As the system took on more definite form the author began

to present to his seminar groups from time to time somewhat formalized statements regarding special problems. These statements were made in the form of mimeographed memoranda which were later assembled and substantially bound. Four separate volumes of such bound memoranda are on deposit in the Yale University library and in the libraries of several other universities where a certain amount of interest has been manifested in such matters. The bound memoranda extend over the academic years 1936-1938, 1939-1940, 1940-1944, and 1945-1946 (8, 3, 4, 6).

From time to time during the compilation of the first three of these volumes the principles which seemed most promising at the moment were gathered together in a numbered series, sometimes accompanied by a few deductions based on the postulates in question. The dates of the chief of these series follow, with the pages of the volume where they may be found:

December 2, 1937, pp. 115-116.

May 3, 1939, pp. 24-29.

January 27, 1940, pp. 40-41; 45; 47-48.

February 10, 1940, pp. 49-54.

February 12, 1940, pp. 55-59.

April 30, 1940, pp. 111-117.

July 8, 1941, pp. 44-52.

December 28, 1942, pp. 163-166.

July 13, 1943, pp. 167-170.

December 20, 1943, pp. 176-177.

On September 4, 1936, the writer used a much abbreviated version of an earlier set of postulates which was substantially like that listed above under date of December 2, 1937, as the basis of an address given by him as retiring president of the American Psychological Association. This was published in routine form (2) in January, 1937. The next published version of the system appeared in the

joint volume, *Mathematico-Deductive Theory of Rote Learning* (9). The most recently preceding published form is presented in the bold-faced type sections scattered through the volume, *Principles of Behavior* (5).

During the years since the publication of *Principles of Behavior* numerous reasons for changes and modifications in the system as there presented have been revealed. These arose through a careful study of experimental results from Yale laboratories and from laboratories in other institutions, numerous thoughtful theoretical criticisms, and the writer's use of the postulates of the system in making concrete deductions of the systematic details of individual (non-social) behavior in connection with a book which he is now writing on this subject. As a consequence, the mathematical aspects of many of the postulates have been formulated, or reformulated, and the verbal formulation of nearly all has been modified to a certain extent. One postulate (5, p. 319) has been dropped in part as empirically erroneous, some postulates have been divided, and others have been combined; several new postulates have been added, and a number of the original postulates have been deduced from others of the present set and now appear as corollaries. The net result is an increase of from sixteen to eighteen postulates, with twelve corollaries.

The same sort of revision as that just described is certain to be necessary in the case of the present set of behavior postulates and corollaries. This is true of all natural-science theories; they must be continually checked against the growing body of empirical fact. In order to facilitate this winnowing and expanding process, the postulates and corollaries as of November, 1949, are here presented in an unbroken sequence.

POSTULATE I

*Unlearned Stimulus-Response
Connections (sU_R)*

Organisms at birth possess receptor-effector connections (sU_R) which under combined stimulation (S) and drive (D) have the potentiality of evoking a hierarchy of responses that either individually or in combination are more likely to terminate the need than would be a random selection from the reactions resulting from other stimulus and drive combinations.

POSTULATE II

*Molar Stimulus Traces (s') and Their
Stimulus Equivalents (S')*

A. When a brief stimulus (S) impinges on a suitable receptor there is initiated the recruitment phase of a self-propagating molar afferent trace impulse (s'), the molar stimulus equivalent (S') of which rises as a power function of time (t) since the termination of the stimulus, *i.e.*,

$$S' = At^a + 1.0,$$

S' reaching its maximum (and termination) when t equals about .450".

B. Following the maximum of the recruitment phase of the molar stimulus trace, there supervenes a more lengthy subsident phase (s'), the stimulus equivalent of which descends as a power function of time (t'), *i.e.*,

$$S' = B(t' + c)^{-b},$$

where $t' = t - .450''$.

C. The intensity of the molar stimulus trace (s') is a logarithmic function of the molar stimulus equivalent of the trace, *i.e.*,

$$s' = \log S'.$$

POSTULATE III

Primary Reinforcement

Whenever an effector activity (R) is closely associated with a stimulus

afferent impulse or trace (s') and the conjunction is closely associated with the diminution in the receptor discharge characteristic of a need, there will result an increment to a tendency for that stimulus to evoke that response.

Corollary i

Secondary Motivation

When neutral stimuli are repeatedly and consistently associated with the evocation of a primary or secondary drive and this drive undergoes an abrupt diminution, the hitherto neutral stimuli acquire the capacity to bring about the drive stimuli (S_D) which thereby become the condition (C_D) of a secondary drive or motivation.

Corollary ii

Secondary Reinforcement

A neutral receptor impulse which occurs repeatedly and consistently in close conjunction with a reinforcing state of affairs, whether primary or secondary, will itself acquire the power of acting as a reinforcing agent.

POSTULATE IV

The Law of Habit Formation (sH_R)

If reinforcements follow each other at evenly distributed intervals, everything else constant, the resulting habit will increase in strength as a positive growth function of the number of trials according to the equation,

$$sH_R = 1 - 10^{-a\dot{N}}.$$

POSTULATE V

Primary Motivation or Drive (D)¹

A. Primary motivation (D), at least that resulting from food privation, consists of two multiplicative components, (1) the drive proper (D') which is an increasing monotonic sig-

¹ This postulate, especially parts A, B, and C, is largely based on the doctoral dissertation of H. G. Yamaguchi (11). It is used here by permission of Dr. Yamaguchi.

moid function of h , and (2) a negative or inaction component (ϵ) which is a positively accelerated monotonic function of h decreasing from 1.0 to zero, i.e.,

$$D = D' \times \epsilon.$$

B. The functional relationship of drive (D) to one drive condition (food privation) is: from $h = 0$ to about 3 hours drive rises in an approximately linear manner until the function abruptly shifts to a near horizontal, then to a concave-upward course, gradually changing to a convex-upward curve reaching a maximum of 12.3σ at about $h = 59$, after which it gradually falls to the reaction threshold (sL_R) at around $h = 100$.

C. Each drive condition (C_D) generates a characteristic drive stimulus (S_D) which is a monotonic increasing function of this state.

D. At least some drive conditions tend partially to motivate into action habits which have been set up on the basis of different drive conditions.

POSTULATE VI

Stimulus-Intensity Dynamism (V)

Other things constant, the magnitude of the stimulus-intensity component (V) of reaction potential (sE_R) is a monotonic increasing logarithmic function of S , i.e.,

$$V = 1 - 10^{-a \log S}.$$

POSTULATE VII

Incentive Motivation (K)

The incentive function (K) is a negatively accelerated increasing monotonic function of the weight (w) of food given as reinforcement, i.e.,

$$K = 1 - 10^{-a \sqrt{w}}.$$

POSTULATE VIII

*Delay in Reinforcement (J)*²

The greater the delay in reinforcement, the weaker will be the resulting reaction potential, the quantitative law being,

$$J = 10^{-jt}.$$

POSTULATE IX

The Constitution of Reaction Potential (sE_R)

The reaction potential (sE_R) of a bit of learned behavior at any given stage of learning is determined (1) by the drive (D) operating during the learning process multiplied (2) by the dynamism of the signaling stimulus at response evocation (V_2), (3) by the incentive reinforcement (K), (4) by the gradient of delay in reinforcement (J), and (5) by the habit strength (sH_R), i.e.,

$$sE_R = D \times V \times K \times J \times sH_R.$$

where

$$sH_R = sH_R \times V_1$$

and V_1 represents the stimulus intensity during the learning process.

Corollary iii

The Behavioral Summation (+) of Two Reaction Potentials, sE_R and sE'_R

If two stimuli, S' and S , are conditioned separately to a response (R) by N' and N reinforcements respectively, and the sE_R generalizes from S' to S in the amount of sE_R , the summation (+) of the two reaction potentials at S will be the same as would result for the equivalent

² It is probable that this postulate ultimately will be deduced from other postulates, including II B and VI; thus it will become a corollary. In that case the phenomena represented by J would be taken over in IX by sH_R , just as sH_R now is.

lent number of reinforcements at S , i.e.,

$$sE_R + s\bar{E}_R = sE_R + s\bar{E}_R - \frac{sE_R \times s\bar{E}_R}{M},$$

where M is the asymptote of sE_R .

Corollary iv

The Withdrawal of a Smaller Reaction Potential (sE_R) from a Larger One (C)

If $C = sE_R + sE'_R$, then

$$sE_R = \frac{M(C - sE'_R)}{M - sE'_R}.$$

Corollary v

The Behavioral Summation ($+$) of Two Habit Strengths, sH_R and $s\bar{H}_R$

Since the asymptote of sH_R is 1.0,

$$sH_R + s\bar{H}_R = sH_R + s\bar{H}_R - sH_R \times s\bar{H}_R.$$

Corollary vi

The Withdrawal of a Smaller Habit Strength (sH_R) from a Larger One (C)

If

$$C = sH_R + sH'_R,$$

then

$$sH_R = \frac{C - sH'_R}{1 - sH'_R}.$$

Corollary vii

The Problem of the Behavioral Summation ($+$) of Incentive Substances (K)

If two incentive substances, f and a , have as the exponential components of their respective functional equations $A\sqrt{w}$ and $B\sqrt{m}$, the second substance will combine ($+$) with the first in the production of the total K by taking as the exponent of the new formula: the simple addition of the units of the first substance to the product of the units of the second substance multiplied by the quotient obtained by dividing the square of the numerical portion of the second

exponent by the square of the numerical portion of the first exponent, i.e., in regard to exponents,

$$w + m = w + m \times \frac{B^2}{A^2}.$$

Corollary viii

The Problem of the Behavioral Summation ($+$) of Stimulus-Intensity Dynamism (V)

If two stimulus aggregates (S and S') are each scaled in terms of the absolute threshold of the stimulus in question for the subject involved, the stimulus-intensity dynamism (V) of the compound will be the simple summation of the scaled intensity values as substituted in the equation, i.e.,

$$V_S + V_{S'} = 1 - 10^{-v \log(S+S')}.$$

POSTULATE X

Inhibitory Potential

A. Whenever a reaction (R) is evoked from an organism there is left an increment of primary negative drive (I_R) which inhibits to a degree according to its magnitude the reaction potential (sE_R) to that response.

B. With the passage of time since its formation, I_R spontaneously dissipates approximately as a simple decay function of the time (t) elapsed, i.e.,

$$I'_R = I_R \times 10^{-at}.$$

C. If responses (R) occur in close succession without further reinforcement, the successive increments of inhibition (ΔI_R) to these responses summate to attain appreciable amounts of I_R . These also summate with sI_R to make up an inhibitory aggregate (I_R), i.e.,

$$I_R = I_R + sI_R.$$

D. When experimental extinction occurs by massed practice, the I_R

present at once after the successive reaction evocations is a positive growth function of the order of those responses (\dot{n}), *i.e.*,

$$\dot{I}_R = a(1 - 10^{-b\dot{n}}).$$

E. For constant values of super-threshold reaction potential (sE_R) set up by massed practice, the number of unreinforced responses (n) producible by massed extinction procedure is a linear decreasing function of the magnitude of the work (W) involved in operating the manipulanda, *i.e.*,

$$n = A(a - bW).$$

Corollary ix

Stimuli and stimulus traces closely associated with the cessation of a given activity, and in the presence of appreciable I_R from that response, become conditioned to this particular non-activity, yielding conditioned inhibition (sI_R) which will oppose sE_R 's involving this response, the amount of ΔsI_R generated being an increasing function of the I_R present.

Corollary x

For a constant value of n , the inhibitory potential (\dot{I}_R) generated by the total massed extinction of reaction potentials set up by massed practice begins as a positively accelerated increasing function of the work (W) involved in operating the manipulandum, which gradually changes to a negative acceleration at around 80 grams, finally becoming asymptotic at around 110 grams.

Corollary xi

For a constant value of the work (W) involved in operating the manipulandum, the inhibitory potential (\dot{I}_R) generated by the massed total extinction of reaction potentials set up by massed practice is a negatively accelerated increasing function of the total number of reactions (n) required.

POSTULATE XI

Stimulus Generalization ($s\bar{H}_R$, sE_R , and sI_R)

A. In the case of qualitative stimuli, S_1 and S_2 , the effective habit strength ($s\bar{H}_R$) generates a stimulus generalization gradient on the qualitative continuum from the simple learned attachment of S_1 to R :

$$s_2\bar{H}_R = s_1\bar{H}_R \times 10^{-ad},$$

where d represents the difference between S_1 and S_2 in j.n.d.'s, and

$$s_2E_R = D \times V \times K \times J \times s_1\bar{H}_R,$$

where $D \times V \times K \times J$ are constant.

B. A stimulus intensity (S_1) generalizes to a second stimulus intensity (S_2) according to the equation,

$$s_2\bar{H}_R = s_1\bar{H}_R \times 10^{-bd},$$

where d represents the difference between $\log S_1$ and $\log S_2$ and

$$s_2E_R = (s_1\bar{H}_R \times V_2)(D \times K \times J),$$

where $(D \times K \times J)$ are constant and V_2 is the stimulus-intensity dynamism of S_2 .

C. In the case of qualitative stimulus differences, ordinary conditioning and extinction spontaneously generate a gradient of effective inhibitory potential (sI_R) which is a negative growth function of sI_R and d , *i.e.*,

$$s_2I_R = s_1I_R \times 10^{-ad}$$

and in the case of stimulus-intensity differences,

$$s_2I_R = s_1I_R \times 10^{-bd} \times V_2.$$

Corollary xii

When a habit is set up in association with a given drive intensity and its strength is tested under a different drive intensity, there will result a falling gradient of $s\bar{H}_R$ and sE_R .

POSTULATE XII

Afferent Stimulus Interaction

All afferent impulses (s 's) active at any given instant mutually interact, converting each other into \dot{s} 's which differ qualitatively from the original s 's so that a reaction potential (sE_R) set up on the basis of one afferent impulse (s) will show a generalization fall to sE_R when the reaction (R) is evoked by the other afferent impulse (\dot{s}), the amount of the change in the afferent impulses being shown by the number of j.n.d.'s separating the sE_R 's involved according to the principle,

$$d = \frac{\log \frac{sE_R}{\dot{s}E_R}}{J}.$$

POSTULATE XIII

Behavioral Oscillation

A. Reaction potential (sE_R) oscillates from moment to moment, the distribution of sO_R deviating slightly from the Gaussian probability form in being leptokurtic with β_2 at about 4.0, i.e., the form of the distribution is represented by the equation,³

$$y = y_0 \frac{1}{\left(1 + \frac{x^2}{a^2}\right)^m}.$$

B. The oscillation of sE_R begins with a dispersion of approximately zero at the absolute zero (Z) of sH_R , this at first rising as a positive growth function of the number of subthreshold reinforcements (N) to an unsteady maximum, after which it remains relatively constant though with increasing variability.

C. The oscillations of competing reaction potentials at any given instant are asynchronous.

³ This equation is taken from 10, p. lxiii.

POSTULATE XIV

Absolute Zero of Reaction Potential (Z) and the Reaction Threshold (sL_R)

A. The reaction threshold (sL_R) stands at an appreciable distance (B) above the absolute zero (Z) of reaction potential (sE_R), i.e.,

$$sL_R = Z + B.$$

B. No reaction evocation (R) will occur unless the momentary reaction potential at the time exceeds the reaction threshold, i.e., unless,

$$\dot{s}\ddot{E}_R > sL_R.$$

Corollary xiii

The Competition of Incompatible Reaction Potentials ($s\ddot{E}_R$)

When the net reaction potentials ($s\ddot{E}_R$) to two or more incompatible reactions (R) occur in an organism at the same instant, each in a magnitude greater than sL_R , only that reaction whose momentary reaction potential ($s\ddot{E}_R$) is greatest will be evoked.

POSTULATE XV

Reaction Potential (sE_R) as a Function of Reaction Latency (st_R)

Reaction potential (sE_R) is a negatively accelerated decreasing function of the median reaction latency (st_R), i.e.,

$$sE_R = a st_R^{-b}.$$

POSTULATE XVI

Reaction Potential (sE_R) as a Function of Reaction Amplitude (A)

Reaction potential (sE_R) is an increasing linear function of the Tarchanoff galvanic skin reaction amplitude (A), i.e.,

$$sE_R = cA.$$

POSTULATE XVII

*Complete Experimental Extinction
(n) as a Function of Reaction
Potential (sE_R)*

A. The reaction potentials (sE_R) acquired by massed reinforcements are a negatively accelerated monotonic increasing function of the median number of massed unreinforced reaction evocations (n) required to produce their experimental extinction, the work (W) involved in each operation of the manipulandum remaining constant, *i.e.*,

$$sE_R = a(1 - 10^{-bn}) + c.$$

B. The reaction potentials (sE_R) acquired by quasi-distributed reinforcements are a positively accelerated monotonic increasing function of the median number of massed unreinforced reaction evocations (n) required to produce their experimental extinction, the work (W) involved in each operation of the manipulandum remaining constant, *i.e.*,⁴

$$sE_R = a \times 10^{bn} + c.$$

POSTULATE XVIII

Individual Differences

The "constant" numerical values appearing in equations representing primary molar behavioral laws vary from species to species, from individual to individual, and from some physiological states to others in the same individual at different times, all

⁴ The equation of XVII B is regarded with more than usual uncertainty. Fortunately the true function can be determined by a straightforward empirical procedure.

quite apart from the factor of behavioral oscillation (sO_R).

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AN INTERPRETATION OF LEARNING UNDER AN IRRELEVANT NEED¹

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INTRODUCTION

Essential differences between reinforcement and nonreinforcement theories of learning appear to center about the definition of one of the experimental variables determining the acquisition of habit strength or cognitions. This variable is *N*, number of reinforcements. According to a sign-Gestalt theory such as Tolman's, cognition is a function of *N* and other variables where *N* may be defined by the operations of presentation of a sign-significate sequence and the counting of such occasions. In a reinforcement theory such as Hull's habit strength is a function of the number of reinforcements (*N*) and other variables. *N* may be defined here by the operations of presentation of a goal situation and the counting of such occasions.³

Pertinent data on the problem of defining *N* are the experimental studies of latent learning which use a single choice point maze. Two procedures have been commonly employed in these studies. In the first procedure *Ss* motivated for a relevant goal object (food or water) have commerce with a goal object (water or food) for which

they are satiated. Test trials are introduced by establishing a need or demand for the previously undesired goal object while satiating for the goal object relevant during training (1, 2, 4, 13, 15). In the second procedure *Ss* are satiated for the goal objects (food and water) for which they will be motivated during the later test trials while activated by a weak irrelevant need. Test trials are introduced by inducing a need for one of the previously undesired goal objects while satiating for the other (3, 8, 14). The present paper will be concerned with this second type of experiment in which *Ss* are activated by irrelevant weak drives which afford nondifferential reinforcement. This design avoids certain objections to studies in which *Ss* are strongly motivated for a single goal object while satiated for another (5).

It has been asserted that when *Ss* are running under fairly strong motivation their field of perception is so narrowed that they do not notice the undesired goal object. Conditions are not optimal for the formation of associations between sign and related goal object. Close temporal contiguity between signs also may not be obtained, since strongly motivated *Ss* will take a longer time to run to the undesired goal box on their forced trials than to the desired goal box on their free choices. It may be argued that in studies employing a strong primary need to motivate the *Ss*, one goal box acquires the differential signification of need satisfaction instead of the signification of either hunger or thirst satisfaction.

¹ This article is part of a dissertation presented in August 1948 to the faculty of the Department of Psychology of the State University of Iowa in partial fulfillment of the requirements for the M.A. degree. The writer wishes to express his appreciation to Professor Kenneth W. Spence for his many helpful criticisms and suggestions.

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³ A reinforcing or goal situation is one which *Ss* seek to attain or preserve, and whose effects result in hypothetical increments of response strength (12).

Spence, Lippitt, and Bergmann attempted to avoid these criticisms by training Ss satiated for food and water while activated by weak irrelevant needs.⁴ The satiated Ss were induced to run by placing them in a social cage with cage mates after each trial.⁵ Following a training period of seven days were two days of test trials, one trial per day, during which the Ss were under a regime of water or food deprivation. The test results showed an increase of 28.5 per cent on the first test day over the first trial of the last training day in the choice of the path leading to the appropriate goal object. This difference is significant beyond the 1 per cent level of confidence. The per cent of correct choices on the second day did not differ significantly from the per cent choice of the correct path during the training period. Evidence of latent learning or a significant increase in the per cent of correct responses on the first test trial was therefore obtained in this study.

Kendler (3) repeated the Spence, Lippitt, Bergmann study activating the Ss by pushing them when they did not run. An increase of 20.8 per cent correct choices on the first test trial as compared with the choices of the correct alley on the last training day was significant at the 5 per cent level of confidence. Evidence of latent learning was therefore not as great as in the previous study.

Another experiment in which Ss were activated by irrelevant needs while satiated for potential goal objects has been conducted by Meehl and McCorquodale (8). Ss satiated for food and water were given four

trials per day for ten days. After the first two runs each day the Ss were returned to their home cages, presumably receiving some form of social reinforcement, before running the last two trials. Results of the first day's test trials in which Ss were motivated for either food or water indicated a significantly greater than chance number of runs to the appropriate goal object. Test runs the following day under reversed drive conditions did not show a choice of the appropriate goal object greater than chance. Some evidence of latent learning was therefore obtained.

Results markedly different from those of the above experiments were obtained in a study by the writer (7). The apparatus employed was a single choice point T-maze with food in one goal box and water in the other. Social cages equipped with transparent one-way doors were attached directly to the maze. It is important to note that in previous studies employing a social incentive this was not the case. The Ss satiated for food and water were given two trials per day, the first free and the second forced to the opposite side, for a period of twenty days. Each S was allowed to remain in the social cage for two minutes after entering from the goal box. Test trials were introduced the day following the fortieth trial by depriving the Ss of either food or water for approximately twenty-two hours while satiating them for the other goal object. It was found that the frequency of correct response on the first test trial did not differ significantly from chance. Performance on the first test trial likewise did not differ significantly from performance on the free trial of the last day of training.

⁴ An irrelevant need presumably is not reduced by the differential goal objects, food or water in the present experiments.

⁵ The effectiveness of a cage mate as an incentive in maze learning is indicated in an experiment conducted by Ligon (6).

AN ANALYSIS OF LEARNING UNDER AN IRRELEVANT NEED

It appears that an explanation of the divergent data of learning under irrelevant needs may be given on the basis of Spence's analysis of stimulus generalization and secondary reinforcement (11) and his formulation of the fractional anticipatory response as presented at the 1941 meeting of the Midwestern Psychological Association (12).

A schematic representation of an experiment at the conclusion of training in which satiated S_s perceived the relevant goal objects when activated by some weak unrelated need may be seen in Fig. 1. As a result of secondary reinforcement and stimulus generalization received in the right goal box, the fractional anticipatory response to water, rg_w , elicited in S_s satiated for that goal object will move forward in the stimulus response se-

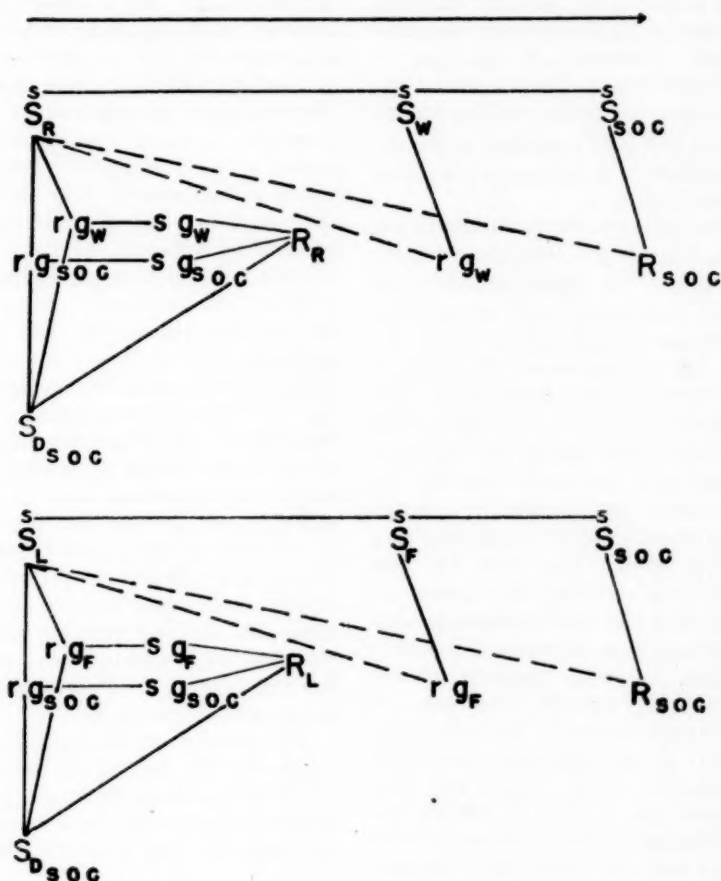


FIG. 1. Diagrammatic representation of the experimental design in which satiated S_s perceive the relevant goal objects (food and water) while activated by a weak unrelated social need. Social reinforcement does not immediately follow perception of the relevant goal object.

quence until it is elicitable by the cues at the start of the right alley.⁶ A similar development of rg_f will occur in the left alley. The hypothetical proprioceptive stimulus, sg_w , aroused by rg_w will become conditioned to the responses of entering the right alley, R_R , leading to water. The proprioceptive stimulus sg_f will become conditioned to the responses of entering the left alley, R_L , leading to food. Assuming that there is some form of social reinforcement, a fractional component of the goal response will also move forward in the stimulus response sequence. However, in the case of the fractional anticipatory social response, rg_{soc_L} and rg_{soc_R} will be identical, since the goal response to the social stimulation is the same after either response choice.

It was noted in the abstracts of the Spence, Lippitt and Bergmann experiment and in the Meehl and McCordale experiment that an appreciable temporal interval occurred between the perception of the relevant goal objects and the securing of reinforcement in the social cage. This delay took place when the Ss were removed from the end boxes by the experimenter and carried to a social cage in another part of the room or a different room. The time between (1) the cues eliciting the turning responses and the cues contiguous with the relevant goal objects was therefore less than the time between (2) the cues eliciting turning responses and cues contiguous with the social goal objects. If the principles of stimulus generalization and secondary reinforcement are invoked, it follows that the stimulus pattern contiguous with a given response will acquire secondary reinforcing properties to the ex-

tent to which it is similar to the stimulus pattern contiguous with the goal object. However, the pattern of external and internal stimuli is changing with time. Consequently, the greater the interval of time elapsing between the events of (1) stimuli contiguous with reinforcement and (2) stimuli contiguous with the responses of turning, the greater will be the difference between the two stimulus patterns; hence, the smaller the amount of generalization of secondary reinforcement. As a result of the different amounts of secondary reinforcement, rg_f will be stronger than rg_{soc} and rg_w will be stronger than rg_{soc} . The excitatory potential of an sg_w determining a right turning response during a test trial under water deprivation will be greater than the excitatory potential of the sg_f determining a left turning response. For a test trial in which the motivation is food deprivation, the excitatory potential of the sg_f determining the left turning responses will be greater than the excitatory potential of the sg_w determining right turning responses.⁷ If performance in this situation is a function of the competition of fractional anticipatory responses, and $rg_w > rg_{soc}$ and $rg_f > rg_{soc}$, then the probability of running right when thirsty is greater than .50 and the probability of running left when hungry is greater than .50. Latent learning would be revealed, as it was in the case of the Spence, Lippitt and Bergmann experiment, and others (3, 8).

The problem at this point is to account for the results of the writer's study, which is similar in design to the Spence, Lippitt and Bergmann study, for example, but markedly different

⁶ Evidence supporting this assumption has been recently obtained by Mr. John Myers (9).

⁷ It is assumed that rg_w is greater than rg_f when an animal is thirsty and rg_f is greater than rg_w when an animal is hungry as a consequence of the past history of the animal.

in the results obtained. This disagreement perhaps may be resolved by an examination of the effects of the different temporal delays involved in the two experiments.

In a manner similar to that in the Spence, Lippitt and Bergmann experiment, rg_f , rg_w , and an undifferentiated rg_{soc} were established (Fig. 2). The hypothetical proprioceptive stimulus sg_w aroused by rg_w was conditioned to the responses of entering the right alley, R_R , leading to water. The hy-

pothetical proprioceptive stimulus sg_f aroused by rg_f was conditioned to the responses of entering the left alley, R_L , leading to food. The proprioceptive stimulus sg_{soc} aroused by rg_{soc} was conditioned to the responses of entering both alleys. In the present experiment the time between (1) the cues eliciting the turning responses and the secondary reinforcement from the relevant goal objects was approximately the same as the time between (2) the cues eliciting the turning re-

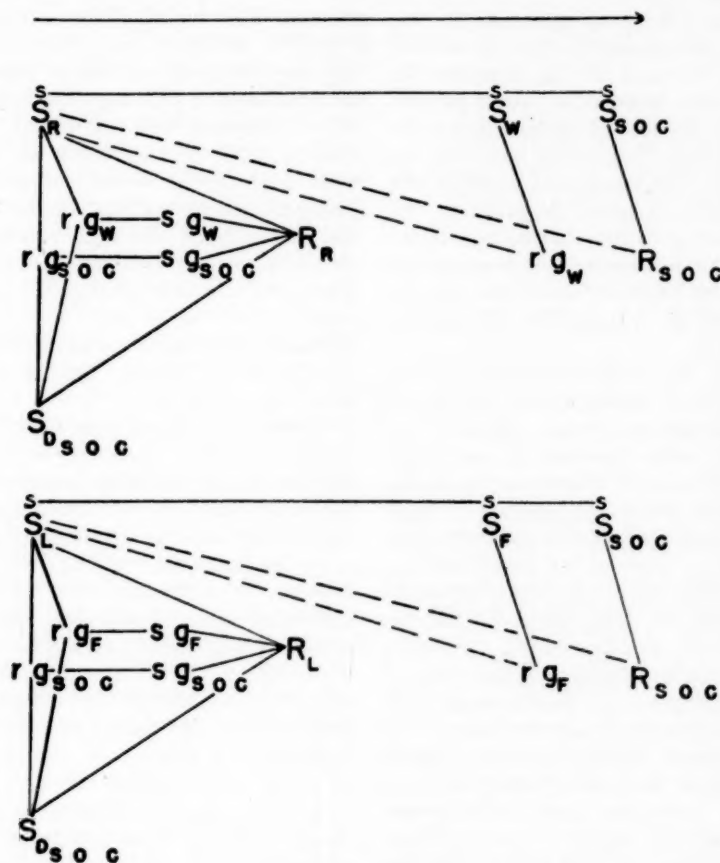


FIG. 2. Diagrammatic representation of the writer's experiment. S_s perceive the relevant goal objects (food and water) while activated by an unrelated social need. Social reinforcement closely follows perception of the relevant goal object.

sponses and the reinforcement from the social goals. The amount of secondary reinforcement that would generalize along the stimulus gradient therefore would be approximately equal. Hence the fractional anticipatory response for water would be approximately equal in strength to the fractional anticipatory response for the social object. Also the fractional anticipatory response for food would be approximately equal in strength to the fractional anticipatory response for the social object.

During a test trial in which the motivation is food deprivation the excitatory potential of the sg_f determining left turning responses would be greater than the excitatory potential of the sg_w determining right turning responses. For a test trial in which the motivation is water deprivation the excitatory potential of the sg_w determining right turning responses would be greater than the excitatory potential of the sg_f determining left turning responses.

Thus, rg_f would compete successfully with rg_w when Ss are motivated for food and rg_w would compete successfully with rg_f when Ss are motivated for water. However, rg_f would dominate the undifferentiated rg_{soc} approximately only half the time when the Ss are deprived of food, and rg_w would dominate rg_{soc} approximately only half the time when Ss are deprived of water. Latent learning would not be apparent in this case.

A consequence of the above analysis is that apparently contradictory data of different latent learning experiments may be derived from a common set of principles and assumptions. That the formulation presented here is more than a simple *ad hoc* construction is indicated by the possibility of deriving a number of testable predictions from it. A general implication

is that in experiments employing irrelevant needs as a means of activating Ss the amount of latent learning will be a function of the relative strengths of the competing fractional anticipatory responses. Thus it would be possible to predict and control the amount of latent learning obtainable by manipulating the variables determining the strengths of the fractional anticipatory responses. One such variable is the time between reinforcement and turning down one alley or the other. In an experiment such as the writer's, latent learning could be obtained as a function of varying intervals of delay imposed after commerce with the goal box and before entrance into the social cage. The strength of the rg for the relevant goal object would remain unchanged under these conditions. But the strength of the rg_{soc} would decrease with increased delay intervals. Therefore the fractional anticipatory response for the relevant goal object would compete with increasing success as the delay between goal box and social cage increased.

Another implication is that latent learning would be inversely related to the strength of the irrelevant need and the magnitude of the irrelevant reward. In the case of the social drive, a preliminary experiment would be necessary to determine whether or not the social drive is a function of amount of isolation from cage mates, or "social deprivation." If the drive is manipulable in this fashion, it would be possible to study the amount of latent learning as a function of the strength of the irrelevant social drive.

It may also be possible to obtain latent learning as a function of the strength of the relevant need during the test trial. It would be necessary first to determine whether the need is an increasing function of the hours of

deprivation to be studied. If this is the case, then it would follow that greater evidence of latent learning would be obtained with increased need states, since the reaction potential of the anticipatory response for the relevant goal object would be increased, hence compete more successfully with anticipatory responses for the irrelevant goal objects.

A possible criticism of the present interpretation is that the concept of fractional anticipatory response is nothing but a translation of Tolman's concept of expectancy; differences are purely verbal. The writer does not believe this to be quite the case. The two concepts are not equivalent, because they differ in their significance. Meaning in this sense, or significance, is acquired by a concept simply as a consequence of its relation to other concepts and laws within a theory (10). A good deal of precise quantitative work has been conducted in the study of secondary reinforcement, a concept used in conjunction with the fractional anticipatory response. This is not the case with respect to the concepts employed in conjunction with expectancy. Hence there would be a loss of meaning if "fractional anticipatory response" were translated as "expectancy" while the converse would not be true.

Because the fractional anticipatory response is a concept within the framework of Hull's theory and expectancy a concept in Tolman's, the two concepts have different meanings and, it is believed, lead to different implications.

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A NOTE ON BROWER'S "THE PROBLEM OF QUANTIFICATION IN PSYCHOLOGICAL SCIENCE"

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In a recent issue of this JOURNAL, Daniel Brower (1) presents several objections to the quantitative orientation characteristic of current psychological activity. It is the purpose of this paper to answer Brower's criticisms and to defend the thesis that the advancement of psychology as a science is, to a significant degree, a function of the quantitative sophistication it achieves.

At the outset, however, the writer would like to stress his basic agreement with the crucial facet of Brower's criticisms: the presence of a disproportionate amount of methodology-oriented energy, with a concomitant sacrifice of more fundamental problems. Methodology and quantification serve no functions in and of themselves. They are but tools to be used in the service of psychological research; they must not and cannot be substituted for the problems themselves. However, it is with several of Brower's criticisms of psychological statistics *per se* and his suggestions for their curtailment that the writer is in disagreement. This paper will be aimed toward those issues.

One of Brower's chief criticisms against quantification is its susceptibility to the "... atomistic fallacy, *i.e.*, contemporary statistics generally deal with parts of phenomena taken out of their natural contexts" (1, p. 326). It is difficult to conceive of controlled observations of complex psychological phenomena in their natural contexts. The clinician would find it highly impractical to observe his client's behavior at home, on the job, in social situations, etc., *i.e.*, in the client's natural context. Obviously this would be prohibitively expen-

sive and time-consuming as well as virtually impossible in our American culture. It would be naive to expect perfect prediction, no matter how complete a "natural context" the scientist chose to use as his area of observation; errors in the measuring instruments, errors due to variability in the individual's responses, and subtle changes in the environment are forces that operate against prediction of unity. Furthermore, perhaps the most fundamental rule of scientific investigation would be violated by this procedure, for observations in natural contexts would preclude the examination of one variable of behavior while others were held reasonably constant. In this respect the so-called atomistic fallacy is an aid, rather than a hindrance, to the understanding of human behavior. Finally there is adequate evidence in sampling theory to support the belief that parts of a phenomenon, when properly selected, are accurate reflections of the whole phenomenon (or natural context).

There have been a few attempts to bring life situations into the laboratory. Role-playing (4) is one of these and perhaps a more dramatic technique is that used by the Office of Strategic Services (10) in the selection of its personnel during the second world war. When perfected these techniques will be extremely valuable. However, they are still parts of phenomena and by no means completely representative of their natural contexts. They are nevertheless capable of yielding useful results and providing the psychologist with helpful hunches for further research.

The atomistic fallacy is oftentimes

fallaciously assumed. A few random lines may be as revealing of the characteristics of an individual's penmanship as several pages or more.

Brower goes on to question the assumption that numbers are more objective than words. He maintains, and correctly so, that interpretation and communication of numeration are always in terms of words. It appears to the writer, however, that the interpretation of words is not as precise and univocal as the interpretation of numbers. Differences between persons and within a person are so varied that even our rich fund of adjectives is an inadequate yardstick by which to gauge these differences. It is not enough, as Brower proposes, that quantification be achieved through the language of relativity, *e.g.*, more than, less than, the same as, more of, fewer of, an increase in, a decrease in, etc. First of all, the language of relativity is necessarily a function of numeration. We do not know that A's test score is greater than B's unless we know the *numerical* scores achieved by A and B. Thus at the outset we see that Brower's language of relativity is quite dependent upon the numeration he would substantially abolish. Moreover, numeration is more univocally meaningful than words of relativity, words which contain different meanings for different people. Brower refers to Carnap and Korzybski as proponents of basic scientific epistemology, but overlooks the fact that these men also espouse the role of *pointing* or *denoting* in the realm of operationism. There is little doubt that numbers are more intrinsically denotative than words. Their specificity is seldom approached by words, even those of extremely low levels of abstraction. The statement, "A's standard score of 2.4 is greater than B's standard score of 1.5," is more denotative and therefore less ambiguous

and more universally meaningful than the statement, "A achieved a score greater than B." In short, the writer holds that quantification should employ both the language of numeration and the language of relativity. Words and numbers are not mutually exclusive. Their joint usage makes the results of psychological research more meaningful, more critical of hypotheses, and therefore more provocative of further research.

Brower asserts that numeration tends to be distortional because of the errors of over- or under-determination of meaning. Over-determination is an error of over-generalization, occurring when one says that a Wechsler-Bellevue I.Q. of 55 proves feeble-mindedness in the individual attaining that score. "By under-determination of meaning we refer to an excessive narrowing of the horizon of application of specific data. For example, an I.Q. of 55 on the Wechsler-Bellevue Adult Intelligence Scale might be misinterpreted in the direction of under-determination of meaning by asserting that this merely shows that the individual has an I.Q. of 55 on the Wechsler test" (1, p. 326). Undoubtedly several psychologists have been guilty of such distortions. However, that is not the fault of numeration so much as it is an indication of an improper interpretation of numeration. Perhaps it is an indictment against the quality and amount of statistical training. But this type of error is probably decreasing as psychology curricula are becoming better planned with respect to courses in statistics and methodology. A disavowal of numeration because of errors by its users is tantamount to an abandonment of atomic research because society has allegedly misused one of the by-products of atomic research.

The frequently quoted dictum, "Figures won't lie, but liars will figure," is

characteristic of the error of over- or under-determination. Moreover, errors of this kind would doubtlessly flood the psychological literature in the *absence* of numeration, because of the greater subjective and connotative qualities inherent in verbal language as opposed to the language of numeration.

In Brower's words, "Contemporary statistics, in becoming increasingly complicated, removes itself more and more from the world of fact which it purports to describe" (1, p. 327). This statement is vigorously challenged on the grounds that the world of fact is probably brought closer with the development of more complicated statistics, mathematics, and logical systems. The word "complicated" has an unfortunate connotation, automatically and implicitly prefaced, perhaps, with the word, "unnecessarily." However, denotatively, "complicated" means "consisting of parts intricately combined; difficult of separation, analysis, solution, etc."¹ But the world of fact actually does consist of parts intricately combined, difficult of separation, analysis, solution, etc. Complicated statistics are not deified as such, but have oftentimes been found necessary for the understanding of a mosaic of complicated phenomena.

The scientist is compelled to employ more complicated techniques if, and only if, the simpler means of analysis fail to produce the desired results. To "fight fire with fire," as it were, it has been found necessary to attack complicated problems with complicated means of solution. The development of the calculus by Newton and Leibnitz, the Quantum Theory by Planck, Einstein's Special and General Theory of Relativity, etc., are all "complicated," but few will argue that they are removed

from the world of fact. The world of fact has been and will probably continue to be a function of "complicated" mathematical and logical expressions. In psychology the invention of factor analysis by Spearman and its refinements by Thurstone and others have undeniably brought us closer to the world of fact. Factor analysis provides the psychologist with a systematic and economical method of interpreting and explaining the multitude of interrelations present in a large table of correlations. It enables the psychologist to produce tests (or test items) that are closer to the *core* of abilities than those purportedly measured by the original tests. There is yet another use for "complicated" methodology. This may be illustrated by Toops' L-Method (6), later modified by Wherry and Gaylord (9). Here is a seemingly complicated technique that serves the extremely useful purpose of appreciably *reducing the time* required to obtain maximal validity with a minimal number of independent variables, when these variables (tests) are given integral gross score weights.

The general characteristics of the population from which a parameter is obtained are called, by Brower, *contextual referents*. He claims that many psychologists have not taken contextual referents into account in the fields of correlation and factor analysis. This may be true, but again, as in the case of the errors of over- or under-determination, this is not a criticism of statistics, but rather an indication of the statistical naiveté of the psychologists he criticizes. Statistical training of young psychologists will sensitize them to the importance of contextual referents in statistical analysis. Furthermore, this training will inevitably result in the development of an increasing number of methodological refinements for measuring contextual referents.

¹ Webster's Collegiate Dictionary (5th edition), 1943.

Brower's criticism of the overuse of linear correlations in psychology is probably justified. However, measures of curvilinear relationships, *etc.*, for example, are available. In addition, regression equations involving independent variables with powers greater than unity are likewise available to the psychologist. That these are not popularly used is not a just condemnation of psychological statistics. Of interest, also, is Brower's censure that in correlations, "... the normality of the distribution of the scores of each variable is taken for granted" (1, p. 329). The Pearson product-moment coefficient, as well as several derivative coefficients, makes no assumptions whatsoever regarding the shape of the distributions (5, p. 109). The only assumption is that of rectilinearity.

It is difficult to conceive of correlational and factorial analyses as removed from the original data, as Brower claims. The writer must reiterate his conviction that these statistical techniques bring the psychologist considerably closer to the rock-bottom data of human behavior. Take, as an illustration, the usefulness of partial correlation as a method of obtaining truer and less contaminated measures of relationship, where the psychologist may examine the relationship between two variables with the effects of other variables removed. Or, in the case of multiple correlation, the psychologist is able to introduce additional behavioral data in order to facilitate greater prediction. With respect to factor analysis, it will be recalled that Wherry and Gaylord (8) have shown that indices of reliability are vastly improvable by the consideration of the factor patterns that reside in a psychological test. Wherry (7) has also demonstrated the efficiency and economy of factor analysis in contrast to Tryon's predictions of the entrance of rats into

the alleys of a maze, predictions based on empirically determined weights. Wherry's results were similar to Tryon's, but were obtained in far less time.

Correlational analysis should be strenuously fortified in psychological science, for it enables the psychologist through the medium of the regression equation (either simple or multiple) to *predict* an individual's behavior in one psychological variable or another. And *prediction* is a cardinal aim of psychology, as well as other sciences.

Only the simpler statistical procedures should be maintained, Brower recommends. Among these he includes measures of significance of the differences between means. The writer seriously wonders whether Brower would continue to entertain tests of significance were he to carry them to their logical position in current statistical practice, *e.g.*, analysis of variance. Analysis of variance is indeed "complicated," and complicated procedures, Brower says, should be abandoned because of their purported distance from the original data.

The writer is in complete sympathy with Brower's plea for a fusion of semantics and quantification, word and number,—two vital tools of human expression. The applications of semantics in psychology, advocated by Wendell Johnson (3) and others, are beginning to take hold. This is also revealed by the interest in operationism by many theoretical psychologists.

By way of summary it may be said that one of Brower's principal criticisms appears to be hurled against the users of statistics. A solution for this problem lies in the hope of more rigorous statistical training among the younger people entering psychology, for "Like a carpenter's saw, statistical methods are only tools, and their results are never any better than the people who employ them" (2, p. 5). Certainly one would

not seriously recommend that certain statistical techniques be cast aside because of an incorrect application of those techniques. This would apply to Brower's error of over- or under-determination, his reference to contextual referents, and his charge that the proper kinds of mathematical relationships (*viz.*, curvilinear rather than rectilinear) are not always used.

The writer has also tried to demonstrate the fallacy of Brower's "atomistic fallacy," by showing that the withdrawal of phenomena from their natural contexts is frequently desirable and, in many instances, unavoidable. In addition, Brower's proposal that the language of relativity be more heavily weighted than the language of numeration is deemed impractical and would probably result in an equivocal interpretation of an individual's behavior. Brower also contends that "complicated" statistics are far removed from the world of facts that they are supposed to describe. Psychologists probably universally abide by Morgan's Canon. However, where simple procedures fail to explain or predict, it is inevitable that more complicated procedures be followed. There is ample evidence of the efficacy of this policy in science generally to justify its adoption in psychology.

In the final analysis it is maintained that quantification, whether simple or

complex, is indispensable in the domain of the construction and verification of hypotheses. The ambivalence of words renders them strikingly unqualified to perform adequately that vital function.

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